

# For Reference

NOT TO BE TAKEN FROM THIS ROOM

Ex LIBRIS  
UNIVERSITATIS  
ALBERTAENSIS













THE UNIVERSITY OF ALBERTA

Towards a Science of Politics: Clearing Away  
Some Underbrush

by



Michael Jackson

A THESIS

SUBMITTED TO THE FACULTY OF GRADUATE STUDIES  
IN PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR THE DEGREE  
OF MASTER OF ARTS

DEPARTMENT OF POLITICAL SCIENCE

EDMONTON, ALBERTA

SPRING, 1971



T 12 11-  
127  
57

UNIVERSITY OF ALBERTA  
FACULTY OF GRADUATE STUDIES

The undersigned certify that they have read, and recommend to the Faculty of Graduate Studies for acceptance, a thesis entitled TOWARDS A SCIENCE OF POLITICS: CLEARING AWAY SOME UNDERBRUSH submitted by Michael W. Jackson in partial fulfillment of the requirements for the degree of Master of Arts.



## ABSTRACT

It is the thesis of this essay that the present debate between the advocates and opponents of a political science qua science is a pseudo-debate. The main question considered is: What is "science" to those who advocate and oppose political science qua science. In connection with this question two main hypothesis will be advanced and examined. The first is that the conceptions of science held by those who advocate and oppose political science qua science are similar. The second hypothesis is that the conceptions of science held by both the advocates and opponents differ markedly from the conceptions of science held by philosophers of science. Neither hypothesis has been disconfirmed. Two features of science contribute to this failure of disconfirmation. First, science like any other activity has two aspects: one that commands the attention of the actor and one that compels the spectator. Second, most political scientists are spectators on natural science which is the basic source of their conceptions of science. Accordingly most political scientists focus on the most visible aspect of science. The most visible aspect of science lies in its context of justification. This context consists of the finished products of scientific research as presented to one's professional colleagues. A conception of science developed by spectators on the basis of this context overlooks a second and less visible context, the context of discovery. It is here that the creative work of science goes on. A conception of science that makes room for this second context is quite different from one which is developed on the basis of the first context alone. A conception of science uninformed by the context of discovery mistakenly sees science as being entirely constituted by a rigid set of procedures for deducing,





generating, or discovering truths. This is the conception of science shared by the advocates and opponents of a scientific political science. It is concluded that the first thing political science must do in order to be scientific is to stop attempting to ape the natural and other sciences.



## TABLE OF CONTENTS

page

### CHAPTER

I.	INTRODUCTION . . . . .	1
	Footnotes . . . . .	6
II.	THE NOTION OF SCIENTIFIC METHOD HELD BY EXPO- NENTS OF THE IDEA OF A POLITICAL SCIENCE. .	8
	Footnotes . . . . .	26
III.	THE NOTION OF SCIENTIFIC METHOD HELD BY THE CRITICS OF THE IDEA OF A POLITICAL SCIENCE	30
	Footnotes . . . . .	54
IV.	SCIENCE, SCIENTIFIC METHOD AND THE PHILOSOPHY OF SCIENCE. . . . .	60
	Footnotes . . . . .	91
V.	POLITICAL SCIENCE, POLITICAL SCIENTISTS AND THE IDEA OF A POLITICAL SCIENCE . . . . .	97
	Footnotes . . . . .	119
	BIBLIOGRAPHY . . . . .	123



## CHAPTER I

### INTRODUCTION

In this essay I do not aim directly at contributing anything new to our store of knowledge of the political process. Accordingly, I will not be concerned with the substantive conceptions or material propositions which the works of a given author may contain. This is an essay in the philosophy of science, more precisely, in the philosophy of political science. Philosophy of science, in this case philosophy of political science, aims only at clarifying what we already know. The knowledge philosophy of science seeks is about science itself and is clarificatory.

The label "philosophy of science" is applied to several undertakings, all "philosophical" in the sense of seeking general knowledge -- in this case about science or about the domain revealed by science. The three main types of undertakings included are (1) the sociology of science, (2) the metaphysics and ontology of science, and (3) the epistemology and logic of science. In this essay no rigorous attempt has been made to rule out types (1) and (2) where they have seemed useful, but for the most part I have tried to limit myself to considerations of type (3) only. Considerations of type (1) and (2) are treated where they seem useful in the consideration of issues of type (3). It should be emphasized that this essay makes no claim to being itself a philosophy of political science. Insofar as it is an essay in the philosophy of political science, it seeks general knowledge about political science alone, not knowledge of the domain revealed by political science. The domain of the essay, then, is limited to less than would be required by a comprehensive philosophy of political



science. Moreover, my pursuit of such general knowledge in these pages will be largely negative and critical, suggesting some characteristics a scientific political science would not have;<sup>1</sup> it is not positive or creative as would be expected of a developed philosophy of political science.

While there is a weighty literature on methodology in social science in general and political science in particular, there is only a sparse literature in the philosophy of social science in general and political science in particular.<sup>2</sup> This paucity is both a challenge and burden to someone puzzled by philosophy of political science kinds of problems. It allows one considerable freedom to work on any number of problems from a wide variety of points of view. But it also makes it impossible to rely on and work from the convenience of a commonly understood point of view, because few have been established in this area.

In 1968 Eulau could write happily that infighting over the nature of political science was at an end, so that research and reflection on the first order work of political science could proceed now that such second order problems had been surmounted.<sup>3</sup> Yet in little more than a year's time the President of the American Political Science Association was forced to contradict Eulau.<sup>4</sup> Easton wrote of a new revolution in political science challenging the now established behavioral revolution whose victory Eulau had only just celebrated. As a result of this new revolution, North American political science is in a mood of introspection.<sup>5</sup> The formations of the Caucus for a New Political Science and the Conference for a Democratic Politics, in addition to an increasingly literature on the discipline itself, bear ample witness to this claim.<sup>6</sup> In a way this introspection is a







sign of health. This occasion presents the practitioner of political science with the rare opportunity to publicly re-examine, renew, or, if a need is felt, restructure the whole or some part of his vocation. An undertaking such as this is immense. In this essay I propose to make a beginning in this direction. It seems to me that progress can be made by investigating the controversy over the idea of a political science as science, where possible, focusing on the notions of scientific method involved in such contemplations.

The spatial and temporal dimensions of this essay will be American, and Canadian, political science since 1960.<sup>7</sup> I will consider the writings of those who advocate and criticize the development of a scientific political science. Among the advocates I will limit myself to writers widely referred to as behavioralists, methodologists, and empirical theorists, leaving aside for the most part students of international politics, game theory and the like. The limitations of my treatment of critics are more detailed and is best left to the beginning part of Chapter Three where they are treated.

Though it is doubtful that any of the labels used to characterize political scientists make very much sense at all as they do not predict clearly any consistent set of behaviours on the part of their supposed adherents, still it may be useful for me to say that the advocates I shall be considering are a part of the movement known as the behavioral revolution.<sup>8</sup> For as Dahl has noted, "the 'behavioral approach' might better be called the 'behavioral mood' or perhaps even the 'scientific outlook'".<sup>9</sup> There is, then, an equation of some sort between the terms "behavioralism" and "science" when employed by political scientists in reference to political science.



It is true that the term "behavioralism" has a partisan ring among political scientists. That heterogeneous congregations of innovators are lumped together under this appellation is also true. Yet there is a core of agreement amongst political scientists to whom this label is affixed. It is, as Wolin has said, only to repeat what the behaviorists themselves have said to say that their movement is unified by a concern for method of inquiry.<sup>10</sup>

I shall compare and contrast the idea of political science and the notions of scientific method held by both advocates and critics of scientific political science with aspects of the idea of science held by philosophers of science. It is the thesis of this essay that the debate between the advocates and critics of science in political science is largely a pseudo-debate for they share a view of science that is incompatible with the logic of the views of science held by philosophers of science which are, at least, more nearly right. The views of the philosophers must prevail due to their logical force and by virtue of their authority as students of science. Therefore, regardless of which of the two views of science in political science triumphs, it must be clear that the idea of a political science has not been judged. The error of the advocates is two-step in this pseudo-debate. First, their perception of science is logically problematic at a number of points in view of the arguments of the philosophers of science. Second, science in one field cannot be achieved by emulation of science as practiced in another field. In a way the error of the critics also consists of two-steps. First, their vision of the idea of a political science is mistakenly limited to only a distillation of what is currently practiced under that rubric, with little or no thought to the potentials of science. Second, insofar as they take this distillation to represent science, they share the mistake of the advocates



in their view of the character of science.

To be candid, I must say that I am compelled to make this argument by virtue of the fact that much of the new criticism of political science is, in my view, most persuasive, while the defenses put forward for scientific political science are, too often, inane and disappointing. If the issue of science in political science were to be settled on the basis of the debate between these advocates and opponents the critics might well triumph. In face of this possibility I wish to clarify, in part, just exactly what the debate is and is not about. I wish to prevent the baby of science from being thrown out with the bath water of this pseudo-debate.

What follows will be divided into four chapters.

I shall first characterize the views of the advocates of the idea of a political science on science and scientific method. This is Chapter Two.

I shall then characterize the views of some of the critics of the idea of a political science on science and scientific method. This is Chapter Three.

I shall then look at aspects of the idea of a science and the notion of scientific method as seen by some philosophers of science. This is Chapter Four.

I shall then compare and contrast the three images of science and scientific method seen in the preceding three chapters. This is Chapter Five.





## FOOTNOTES, CHAPTER I

<sup>1</sup>Before proceeding it may prove useful to pause for a word on terminology. To express the conception of a science of politics I shall use a number of terms more or less interchangeable: the idea of a political science, political science qua science, scientific and behavioral political science (or behavioralism for short). Hopefully the use of such an array of terms will provide without causing confusion some relief from the repeated references to the concept which I shall need to make. Each term refers not to one clearly defined concept, but to a series of notions held by those who support the development of a political science emphasizing "science".

In this note I also want to say a word about the term "natural science" which I shall need to use occasionally. The use of "natural science" as contrasted to or mutually exclusive of "social science" is wholly illogical. I am unable to conceive of a science which is not "natural" and I must insist on the naturalness of social phenomena. For convenience however, "natural science" is used in the present essay in accordance with its ordinary usage. It is used to refer to the sciences which have for subject matter phenomena of inorganic or organic character exclusive of that which is social and the subject matter of the social sciences.

<sup>2</sup>General works on the philosophy of political science include E. Meehan, The Theory and Method of Political Analysis (Homewood, Illinois: Dorsey Press, 1965) and Explanation in Social Science, (Homewood, Illinois: Dorsey Press, 1968) V. Van Dyke, Political Science (Stanford: Stanford University Press, 1960), and W. Runciman, Political Theory and Social Science (Cambridge: Cambridge University Press, 1963).

<sup>3</sup>H. Eulau, "Political Behavior", D. Sills, ed., International Encyclopedia of Social Sciences, XII (New York: Macmillan and the Free Press, 1968), p. 211.

<sup>4</sup>D. Easton, "The New Revolution in Political Science", American Political Science Review, LXIII (1969), pp. 1051-1061. (This address was first given as the invited Presidential address at the Canadian Political Science Association meetings, June 1969 in Toronto).





<sup>5</sup>Though dissatisfaction with contemporary political science is not exclusively American. Note for example B. Crick, The American Science of Politics (London: Routledge and Kegan Paul, 1959) and A. Lijphart, "Political Science versus Political Advocacy," Acta Politica, V (1970) pp. 165-171.

<sup>6</sup>I forego reference to the kind of literature I have in mind at this time on the view that the pages which follow in this essay provide this detail in a more coherent form than would be possible here.

<sup>7</sup>Insofar as political science is primarily an American discipline specific attention to American political science is useful. The controversy prior to 1960 has been, more or less, exhaustively treated already by A. Kalleberg, An Analysis of the Nature and Validity of the Idea of a Science of Politics in Recent Political Theory, (Minneapolis: University of Minnesota, unpublished PhD dissertation, 1960).

<sup>8</sup>See R. Dahl who writes,

"The behavioral approach is an attempt to improve our understanding of politics by seeking to explain the empirical aspects of political life by means of methods, theories and criteria of proof that are acceptable to the canons, conventions and assumptions of modern empirical science."

"The Behavioral Approach in Political Science", American Political Science Review, LV (1961), pp. 763-772; reprinted in N. Polsby, R. Dentler and P. Smith, eds., Politics and Social Life (Boston: Houghton Mifflin, 1963), p. 19.

It is partially on the basis of this statement that I have chosen to consider empirical theorist, methodologists and behavioralists (the ever-present residual category).

<sup>9</sup>Dahl, "The Behavioral Approach", p. 21.

<sup>10</sup>See also S. Wolin, "Political Theory as a Vocation", LXIII (1969), p. 1063; G. Almond and S. Verba, The Civic Culture (Princeton: Princeton University Press, 1963), p. 43; D. Easton, "Political Science", D. Sills, ed., International Encyclopedia of the Social Sciences, XII (New York: Macmillan and the Free Press, 1968), p. 296; and F. Pinner, "Notes on Method in Social and Political Research," Polsby, Dentler and Smith, eds., Politics and Social Life, p. 145.



CHAPTER TWO  
THE NOTION OF SCIENTIFIC METHOD  
HELD BY EXPONENTS OF THE IDEA OF A POLITICAL SCIENCE

The adoption of a method of inquiry is important business. It is a meta-research decision. As such, it is a part of one's assumptions, or first principles which cannot themselves be proven by research undertaken on its own basis, for to do so would be circular. The answers arrived at depend in considerable part on the questions asked and how they are asked. Such assumptions of necessity exclude certain kinds of conclusions. This is true of all manner of inquiry whether scientific or not. It is most noticeable in the more formal methods of analysis of statistics and logic. Within each of these techniques, results to a certain extent are the consequences of the technique itself. Technique determines to some extent what kinds of data are made and how the data are manipulated in analysis.<sup>1</sup> It determines the making of data by defining the range of phenomena which one will be prepared to consider as potentially convertible into data. It determines analysis of data by specifying a range of acceptable manipulations of the data. Insofar as methodology deals with the theory of knowledge as applied to a particular kind of phenomena it can be equated with epistemology.<sup>2</sup> Method stipulates standards, or norms, for much of the character of data and analysis.<sup>3</sup> In so doing, certain sense phenomena are ruled out of consideration for inclusion as data and certain manipulative techniques are rendered impossible. It is no wonder then that it can be thought of as affecting results, for method is supposed to have a systematic effect on the process of inquiry and analysis. In my judgment then, it seems reasonable to expect that methods of inquiry and analysis should be carefully considered before adoption.





In this chapter I shall try to characterize the understanding of scientific method and in turn that of science, adopted as a method of inquiry by a range of contemporary exponents of behavioral (scientific) political science. I will restrict my attention to three main sorts of political scientists: those regarded as political behavioralists such as Eulau, those regarded as empirical theorists such as Easton and those regarded as methodologists such as Golembiewski. I shall also consider one particular sort of political science literature namely textbooks for the first undergraduate and graduate courses. In selecting books for treatment here I shall of course look for those with a behavioral, empirical theoretical or methodological emphasis in so far as this is possible. In this way I hope to cover the core of the scientific movement in political science. I will also refer primarily, but not exclusively, to material published since 1960. I do so because Kalleberg provides an exhaustive account of the literature on the idea of a political science up to 1960.<sup>4</sup> My references to pre-1960 writings will generally be for the purpose of preserving continuity in an individual's thought, like Easton, or for the purpose of including a particularly influential writer or book, as with Lasswell and Brecht respectively.

In what follows I shall first examine discussions of scientific method by avowed advocates of the idea of a political science, noting in particular the vagueness of many pleas for political science qua science. Secondly, I shall turn to a selected number of books designed for the introductory undergraduate course and the basic graduate course in scope and method. In the third section of the chapter, several more detailed attempts to specify the scientific method and its applicability to politics by this group will be examined, focusing particularly on the view



that science is empirical and observational. Finally, I shall examine two characteristics of science, its explicitness and its accumulativeness, which are seen by those writers discussed as stemming from its (definitional) empiricism. I will attempt to demonstrate throughout the chapter that scientific method, and in turn, science, are held by these writers to be fixed and unproblematic as rules for inquiry in the natural sciences and further, that it is assumed that scientific method can be transferred directly from the natural sciences to political science.

Because most of the advocates herein considered are primarily researchers and not philosophers of science it may be supposed that they have derived their perspectives on the philosophy of political science from the writings of others on natural sciences and the philosophy of science and not as a result of their own work or interest in the philosophy of political science. Such borrowing, as all borrowing, is of necessity piecemeal. In considering such borrowing, it might be well to recall Toulmin's remark that every activity has two aspects: the one which engrosses the person engaged in the activity, and the one that engages the outside observer who does not himself participate in the activity.<sup>5</sup> Therefore the care with which political scientists have borrowed from the natural scientist requires examination, for political scientists are outside observers of science trying to become involved in the activity of science and they may be seeing science differently from the outside than the scientist or philosopher of science sees it.

It should be noted here that my interest in that which is omitted through vagueness does not represent an adoption of Strauss's canon of interpretation that omissions are deliberate and therefore significant. Rather, I take





omissions only as a sign of lack of interest in that which is omitted. As such, omissions are revealing of the understanding of the subject matter held by the exponent. It is of course necessary to establish that this or that omission is, in fact, significant. The mere fact of omission is not such proof. My position, unlike that of Strauss, does not therefore require the imputation of intentions.

Lasswell, one of the founding fathers and leading figures of contemporary behavioral political science, gives every impression of having a high regard for scientific method. He is a spokesman of the desirability and the possibility of making political science scientific. For instance, in his Power and Personality<sup>6</sup> he applauds the great virtues of "the scientific method"<sup>7</sup> and "scientific thinking"<sup>8</sup> which he implies may be borrowed wholesale from "physical scientists and engineers."<sup>9</sup> Unhappily my reading of the other two hundred and forty-one pages of this volume, combined with a scrutiny of the index, offers no suggestion as to what "the scientific method" or "scientific thinking" comprises.

Lasswell does not specify how he understands the notion of scientific method, though he is a firm believer in its virtues. Neither does he concern himself with the methodological and philosophy of science concerns of a political science. I can only echo Crick, "I am not purposefully concealing his writings on the philosophy of science. They do not exist."<sup>10</sup>

Among the most vocal champions of political science qua science is Easton. He assails political scientists for their failure to pursue the achievement and implementation of scientific method with sufficient vigor.<sup>11</sup> For Easton,



a commitment "to a science of politics modelled after the methodological assumptions of the natural sciences" is necessary.<sup>12</sup> Like Lasswell, Easton does not deal in detail with the consideration of methodology and philosophy of science in which it would be necessary to base his judgements if they were to be persuasive.

Although such examples of discussions urging or defending the adoption of the idea of a political science without consideration as to what such a view consists of and implies could be multiplied, I will mention only one further illustration.<sup>13</sup>

Deutsch has contended that the test of research methods is the confrontation of data generated by different methods and not the logical analysis of the different methods.<sup>14</sup> The method producing the best data will be revealed in this confrontation says Deutsch.<sup>15</sup> Deutsch fails to appreciate that different methods or techniques produce data of different kinds, as well as data of different qualities. Technique X does not produce a better, more reliable and valid, set of data of kind b than technique Y, rather, it produces a different kind of data, a. If this is the case, then the choice between techniques is in effect a choice between different kinds of data. The solution to such a choice will not emerge from a confrontation of the data from different techniques, for this is a comparative choice requiring that all of the data be of the same kind to be comparable. Only then would a technique yielding "the best data" be a meaningful idea. The choice would then be easy, for if techniques X and Y both produced data of kind a, the qualities of the data Xa and Ya could be compared. The data that performs best on tests of reliability and validity could then be easily determined. But when





techniques X and Y produce data of kinds a and b respectively then such comparisons are not possible. (To make them comparable the data of kinds a and b would have to be characterized with the view of arriving at common characteristics for them and then comparing them on these common characteristics. This is not the solution, for it is simply the creation of new data, namely the common characterizations of data a and b and thus begs the original question.)

The lack of qualification and specification placed upon the notion of scientific method in political research by Lasswell, Easton and Deutsch leads me to infer that, in their views, it is fixed and unproblematic in natural science and may be transferred to political science without difficulty.

One extremely important place to look for views on the idea of a political science and scientific method is in textbooks intended for either the undergraduate introductory course or the graduate scope and methods course. The introductory course gives political science its widest exposure amongst the general populace, while the scope and methods course is generally required of all political scientists during their graduate school career. The images of the idea of a political science that are conveyed by textbooks for these two courses therefore, have the widest impact both within the discipline and among the public at large. Such textbooks are accordingly of special interest in this essay. I make no claim that all textbooks for these two courses convey the same image of a political science as the few textbooks considered hereafter. Nor do I claim that these few books are in any specific way representative of the population of textbooks not treated. (Indeed, I do not know upon what basis such a claim to representativeness could be established.)



I do claim, however, that the textbooks I have chosen to treat were not chosen so as to be unrepresentative. I claim further that they have been and are widely used as textbooks for these courses.

Among textbooks designed for the undergraduate introductory course are those of Alfred DeGrazia.<sup>16</sup> DeGrazia might seem to have been in a special position to author a textbook on political behavior for the first course in political science, for while he is not widely known as a researcher himself he is the founder and editor of the first journal devoted exclusively to behavioral social research, The American Behavioral Scientist (formerly Prod). In this position he has no doubt had wide editorial contact with the developing behavioral literature. In such a position he might well have been able to take an informed yet general view. Such potentialities, however are not realized. DeGrazia writes that his "aim in this book is to introduce the citizen who has had no previous training in political science to its proper elements...."<sup>17</sup> He insists that the "main achievements must come in accentuating the science in social science--the method and system...."<sup>18</sup> Therefore, not surprisingly DeGrazia claims that "political science is...scientific method applied to political events."<sup>19</sup>

What can beginners in or visitors to political science in the first course learn of political science qua science from DeGrazia? He uses the phrase "scientific method".<sup>20</sup> It seems clear from this that he takes it to mean something and to be worth using in behavioral political science. Unfortunately, DeGrazia does not allow himself the luxury of commenting on what he understands "the scientific method" to mean. Via DeGrazia's book, neophytes in political science will then, in a sense, be exposed to his conclusions about the notion of scientific method without the benefit of the arguments and evidence which led





him to them.

Another introductory textbook which illustrates this view of scientific method as being unproblematic is by Pennock and Smith.<sup>21</sup> Under a section entitled "Elements of Scientific Method" they write,

We may safely assume that readers of this volume will have some familiarity with scientific method. Its fundamental components as they are traditionally stated comprise (1) observation and collection of data; (2) classification of these data into significant categories; (3) formulation and (4) verification of generalizations stated as laws, trends or tendencies, according to what is justified by the analysis of the information available.<sup>22</sup>

Plainly there is no question for Pennock and Smith as to what the "scientific method" is.

The author of one textbook designed for the graduate scope and methods course notes that "(o)ne of the objectives of this book is to provide a description of scientific method for students of politics."<sup>23</sup> What sort of description does he provide? His position is that to be scientific one must work according to the scientific method. "(A) discipline can be judged scientific if it makes certain assumptions and follows certain principles...".<sup>24</sup> For Isaak following these "principles" yields "scientific knowledge".<sup>25</sup> Science is, in Isaak's view, defined by its having methods of inquiry that yield scientific knowledge.

A final illustration from among textbooks is a second volume designed for the graduate scope and methods course, a recent "methodological primer".<sup>26</sup> It is plain that the authors are advocates of the idea of a political science.<sup>27</sup> Unhappily, they have little to say about the notion of scientific method, or in turn, the idea of a



political science. At one point however, they do pause to discuss science and its "sequences."<sup>28</sup>

The sequences are:

1. The analysis of the particular problem which induces inquiry;
2. The Baconian inductive observation of those data which problem-analysis suggests are relevant;
3. The development of hypotheses suggested by the relevant data; and
4. The experimental or observational test of hypotheses and their logical consequences . . . <sup>29</sup>

These "sequences" are of course equivalent to what other writers whom we have considered thus far take scientific method to be. Earlier in the work the authors warn against "scientism," which "refers to the uncritical extension into the social sciences of habits of thought appropriate only for the natural sciences or (we might add) of habits of thought equally inappropriate for both social and natural sciences."<sup>30</sup> While the authors show restraint toward scientism they commit the same category of error that they suggest it fosters. They hold that,

The disagreement about methods is more apparent than real, however. For an issue exists only on the assumption that there is a method. This assumption is untenable on several grounds....the problematic situation determines the method.<sup>31</sup>

Yet on the preceding page they have asserted a set of "sequences", the word denoting an ordered series, which constitute "the general phases in the scientific handling of any specific problem."<sup>32</sup> Thus they assert a scientific method called by another name. The qualification on their method is apparently that it attempts to combine "Bacon's inductive method; Descartes' deductive method; and the hypothesis-testing method of Cohen and Nagel".<sup>33</sup> Thus in sequence item number two it provides for induction, in item number three, for hypothesis development and in item number

... ..

... ..



four for deduction. Yet this combination cannot be all things to all men because, as I have indicated, the word "sequence" denotes fixed order amongst the sequences. This order then becomes a rigid fourth sort of scientific method and not an open-ended combination of the basic three mentioned. Consequently, though they claim otherwise, it seems that the authors have a notion of scientific method, altered only in name.

In textbooks for the two basic political science courses at the undergraduate and graduate level the notion of scientific method and the idea of a political science are used, but barely discussed or analyzed. I infer from this paucity once again that scientific method is held to be an unproblematic notion of some fixed specification which can be applied in political science to the domain of politics. Such an assumption would then allow Pennock and Smith, as we have seen, to "assume that readers of" their "volume will have some familiarity with scientific method." One of each pair of textbooks treated above began to give some specification to the notion of scientific method, but now I want to consider several more detailed attempts by exponents of a political science. I do so as a preface to an analysis of the notion of scientific method, and in turn of science, held by such advocates.

My first example is drawn, fittingly enough, from work out of a bastion of behavioral political science research--the Survey Research Center. In the introduction to their collection entitled Public Opinion and Political Behavior containing papers by such noted practitioners of political science qua science as Key, Campbell and Rokkan, Dreyer and Rosenbaum detail what in their view behavioral political science is.<sup>34</sup> They write,





The behavioralist seeks to follow a clear and, hopefully, scientific plan in his research. He tries (1) to formulate some question or hypothesis about human behavior which can be tested, (2) to create a technique for investigation which will provide him with the necessary data that relates to his problem and (3) to develop some criteria or standards by which to measure the reliability of his conclusion.<sup>35</sup>

To Dreyer and Rosenbaum, then, scientific method consists in four steps: (1) the formulation of hypothesis, (2) design of tests, (3) collection of data and (4) development of standards for acceptable test performance.

Ulmer, justly well known for his research on the judiciary, reports that his appreciation of scientific method is inspired by that of Brecht, whose notion of scientific method I shall address shortly. Ulmer writes: "(T)he procedures known collectively as the scientific method" involve:

(1) The selection of a topic and the formulation of a specific research problem; (2) The development of one or more hypothesis; (3) The collection of data relevant for the hypothesis; (4) The evaluation of the hypothesis and (5) The integration of the finding with other knowledge.<sup>36</sup>

This statement shares the view that scientific method is an unproblematic and fixed notion, a view which we have seen so often before. Other than that, it is straightforward and requires no further comment at this time.

Both of these last attempts at the characterization of scientific method, as well as those of Pennock and Smith, Golembiewski, Welsh and Crotty, and Dreyer and Rosenbaum have in common a notion that science is grounded in the facts of the world. It is in this vein that Pennock and Smith speak of the "observation and collection of data."



Golembiewski, Welsh and Crotty list the "observation of those data which... are relevant." Dryer and Rosenbaum mention the formulation of questions known to be testable, i.e., testable against the facts of the world. Ulmer includes the "collection of data relevant to the hypothesis."

An important, if not the important, distinguishing characteristic of science is its basis in the facts of the world revealed through observations. This is an established theme in the idea of any science, and has been, therefore, advanced directly by several exponents of the idea of a political science. I now want to examine the notion of observation that seems to be at the bottom of such a view in political science.

With regard to the notion of observation Deutsch and Rieslebach have discussed "testing...by evidence obtained by standardized and impersonally reproducible procedures."<sup>37</sup> I find nowhere a statement of the meanings of such terms as "evidence", "obtained", "standardized", or "impersonally reproducible." It is quite acceptable for a writer to simply assert his own understandings of such terms without arguing for them. It is troublesome for a writer to fail to specify how he understands such terms as Deutsch and Rieslebach have done. The definitions given to such terms may or may not be regarded as problematic and accordingly subject for disputation. This is a secondary issue; its answer can only be determined when the specifications of such terms are known. What those specifications are is the primary issue.

Other writers have given more attention to the twin notions of observation by scientist and that which is observed--namely the facts of the world. As noted in Chapter One,<sup>38</sup> much recent work on the philosophy of political science has been done by Meehan.<sup>39</sup> As is to be





expected in a work on the philosophy of political science, Meehan is prepared to give a much fuller and more careful consideration to such a fundamental part of science as its twin notions of observation and fact than are the other writers whom we have thus far seen. For these other writers such considerations are not their area of primary concern. Their aim is to get beyond these issues as quickly and painlessly as possible rather than searching for assumptions, intricacies and implications. The latter is understandably more properly the concern of a writer in the philosophy of political science.

"Science deals with facts," Meehan tells us, "but facts are not self-evident; the function of an epistemology is to supply the criterion of factual information."<sup>40</sup> Yet the door to a less rigid and programatic understanding of science that this position opens closes quickly. Within the space of two pages Meehan backtracks. He says that,

The hard core of science is an enormous edifice compounded of brute facts and logical inference. Of course, there is more to it than simply mechanical operations directed to the ordering of facts, but the factual logical core is largely responsible for the immense stability of the fundamental body of scientific knowledge.<sup>41</sup>

While facts are "not self-evident," because "an epistemology is" required "to supply the criterion of" what constitutes "factual informations," science is based on "an enormous edifice" consisting, in part, of "brute facts." Once the hurdle of the development of an epistemology is successfully cleared, science may proceed based on the "brute facts." The development of an epistemology is necessary and important. Once it is completed and a "criterion of" what constitutes "factual information" is



established, then science can unproblematically proceed upon the basis the "brute facts", presumably, measuring up to this criterion.

Earlier in this chapter I referred to Brecht's discussion of scientific method. With "minute scholarly detail"<sup>42</sup> Brecht offers one of the most extensive discussions of science and scientific method I have found in the political science literature. He is concerned with "scientific political theory."<sup>43</sup> He spends nearly one hundred pages propounding scientific method.<sup>44</sup> As my purpose in this essay is not exegesis, I do not wish to present and discuss his position in the detail a full treatment would require. Accordingly, I will work primarily from the summary of his position that he supplies.<sup>45</sup>

Brecht takes the scientific method as fait accompli. "In every inquiry--and that means inquiry within the social as well as the natural sciences--Scientific Method concentrates on the following 'scientific actions'...."<sup>46</sup> The scientific method is taken as given. He then lists the steps of scientific method as he sees them (1) observation, (2) description, (3) measurement, (4) acceptance, (5) inductive generalization, (6) explanation, (7) deductive reasoning, (8) testing, (9) correcting, (10) predicting and (11) non-acceptance.<sup>47</sup> Brecht then spends nearly one hundred pages examining the intricacies of these notions.<sup>48</sup>

The first and basic step of observation in Brecht's notion of scientific method is that upon which the remaining ten steps rest. He discusses various problems of observation from the point of view of various schools of sociology of knowledge. This notwithstanding, he maintains that the "transmissibility" of scientific knowledge qua knowledge





differs from the "transmissibility" of other kinds, such as, for instance, claims to moral knowledge. This is because the subject matter of observation is manifest. The parameters of the varieties of its interpretation are therefore, it is assumed, to a considerable degree set by the thing in itself and the shared range of human sense experience.<sup>49</sup>

A convinced adherent to Brecht's understanding of scientific method is Frohock. Science, Frohock suggests,

can be distinguished on the basis of two basic features: the first, the scientific temperament is alleged to be unbiased at the concrete level; second, the conclusions of science are supposed to be capable of verification by an observer who follows the same techniques.<sup>50</sup>

Both of these characteristics rest on those twin notions of observation and fact. "(T)o be unbiased at the concrete level" means that a scientist does not pre-judge sense phenomena. By not so pre-judging he allows all sense phenomena to speak to his senses as freely and equally as possible. This is the notion of observation; one observes without prior judgments if one is to be a scientist. It is within the free market-place of sense phenomena that a scientist makes his decisions. "(V)erification by any observer who follows the same techniques" implies that some facts are there to be seen. As long as the succeeding observer looks in the same place as his predecessor, that is, as long as he uses the same techniques, the same facts will present themselves to him.

At one point Frohock seems to have a reservation about the notion of scientific method. "We need not believe," he writes, "that in the actual practice of science any formalized procedure is rigidly followed."<sup>51</sup>



He then quotes Kaplan quoting Bridgeman's well known dictum that "the scientist has no other method than doing his damndest."<sup>52</sup> Nevertheless, a page later he says,

The only sensible way to take this delineation is as a conceptual idea to which scientists aspired, even if at times obliquely... But while scientists may only aspire to Brecht's neat outline, the idea conveyed by a scientific method is crucial: some agreed upon procedure must standardize scientific inquiry.<sup>53</sup>

For Frohock, scientific inquiry aspires to "Brecht's neat outline."

Seen as stemming from its foundations in observable fact are two characteristics of science. The first is that it strives to be explicit.

McClosky has maintained that an extremely important characteristic of science is that its reasoning process is explicit, and in a sense therefore public.<sup>54</sup> Science achieves this explicitness by virtue of the use of commonly adhered to "scientific procedures."<sup>55</sup> These procedures allow others to perform reliability and validity check on previous work by replicating it.

The second characteristic is that science is cumulative. Eulau maintains that,

A science of politics that deserves its name must build from the bottom up by asking questions that can, in principle, be answered; it cannot be built from the top down by asking questions that, one has reason to suspect, cannot be answered at all, at least not by the methods of science. An empirical discipline is built by the slow, modest and piecemeal cumulation of relevant theories and data.<sup>56</sup>





Eulau is making a number of points in this passage. It is plain that he is of the view that there is scientific method. He says that science is characterized by its methods. In turn these methods allow it to be empirical. More importantly, to Eulau science is the application of the scientific method to the exploration of a domain of sense phenomena. This he sees as an accumulative enterprise. Last but not least, Eulau contends that any and all aspects of political science can be dealt with by political science qua science.<sup>57</sup>

McClosky, like Eulau, insists that one of the most remarkable features of the natural sciences "is that their... methods" contribute "systematic, cumulative research."<sup>58</sup>

The most striking feature of the notion of scientific method, and the idea of political science qua science that it entails for exponents of contemporary behavioral political science is the facility with which it is assumed to have a fixed character in the natural sciences and the supposed ease with which it can be transferred to the domain of political science. Both assumptions seem to be regarded as unproblematic.

In sum, the group considered in this chapter takes science as an inquiry into matters generally regarded as empirical. They equate science with empirical research and empirical research with science. A notion of scientific method is assumed to exist in natural science, a method considered to stem from science's concern with domains of observable fact, that is, sense phenomena. The twin assumptions are made that there are facts, and that such facts are more or less objectively observable. Because science is rooted in observables which are tapped in an





explicit, that is, public and therefore replicable way, it allows for the accumulation of continuously verified knowledge and the discarding of disconfirmed knowledge-claims. Each succeeding place in scientific work may then, it is thought, build from the conclusions of its predecessors.

After a description of contemporary critiques of political science qua science and an examination of the idea of a science held by a number of philosophers of science (Chapters Three and Four), I shall return to the views on science and scientific method I have just described for a critical re-examination (Chapter Five).



## FOOTNOTES, CHAPTER II

<sup>1</sup>On making data, see C. Coombs, A Theory of Data (New York: Wiley, 1964), pp. 2-3 and 6.

<sup>2</sup>See A. Kaplan, The Conduct of Inquiry (San Francisco: Chandler, 1964), p. 20.

<sup>3</sup>Ibid.

<sup>4</sup>A. Kalleberg, The Idea of a Science of Politics.

<sup>5</sup>S. Toulmin, Foresight and Understanding (London: Hutchison, 1961), p. 13.

<sup>6</sup>H. Lasswell, Power and Personality (New York: Viking Books, 1948).

<sup>7</sup>Ibid., p. 121.

<sup>8</sup>Ibid., p. 203.

<sup>9</sup>Ibid., p. 121.

<sup>10</sup>B. Crick, American Science, p. 139.

<sup>11</sup>Easton, The Political System (New York: Knopf, 1953), p. 25.

<sup>12</sup>Easton, A Framework for Political Analysis (Englewood Cliffs, N.J.: Prentice Hall, 1965), pp. 8 and 17. Cf. Easton, "New Revolution," p. 1050, and Easton, Systems Analysis of Political Life (New York: Wiley, 1965), pp. 477-478.

<sup>13</sup>See, for example, D. Easton, "The Current Meaning of Behavioralism," J. Charlesworth, ed., Contemporary Political Analysis (New York: Free Press, 1967), pp. 11, 19 and 26; J. Murphy, Political Theory: A Conceptual Analysis (Homewood, Illinois: Dorsey Press, 1968), pp. 241-242; and R. Handy and P. Kurtz, "Introduction," R. Handy and P. Kurtz, "A Current Appraisal of the Behavioral Sciences," American Behavioral Scientist, VII (1963-1964) Supplement, pp. 6-8.

<sup>14</sup>K. Deutsch, "Recent Trends in Research Methods in Political Science," J. Charlesworth, ed., A Design for Political Science (Philadelphia: American Academy of Political and Social Science, 1966), pp. 157-158.



<sup>15</sup>Ibid., cf. H. Eulau, "Comment on Deutsch," Ibid., p. 181.

<sup>16</sup>A. DeGrazia, Political Behavior and Political Organization, (Revised Edition) (New York: Free Press, 1962).

<sup>17</sup>A. DeGrazia, Behavior, p. 7.

<sup>18</sup>Ibid., p. 47.

<sup>19</sup>Ibid., p. 55.

<sup>20</sup>See, for example, Ibid., pp. 41, 55, 336 and 346.

<sup>21</sup>J. Pennock and D. Smith, Political Science: An Introduction, (New York: Macmillan, 1964).

<sup>22</sup>Ibid., p. 10.

<sup>23</sup>A. Isaak, Scope and Method of Political Science (Howewood, Illinois: Dorsey Press, 1969), p. 57 footnote.

<sup>24</sup>Ibid., p. 25.

<sup>25</sup>Ibid., p. 26.

<sup>26</sup>R. Golembiewski, W. Welsh and W. Crotty, A Methodological Primer for Political Scientist (Chicago: Rand McNally, 1969),

<sup>27</sup>This supposition is made obvious merely by virtue of the fact that they have bothered to write a book for teaching methodology. No one to my knowledge has ever written a methodological primer which was anti-methodological, or anti-scientific. In the current understanding to qualify as a methodological textbook it is necessary to expound a variety of research techniques such as statistics, survey research or interviewing. To qualify as anti-methodological it is necessary to argue against the use of such techniques. It is not surprising that these two requirements have not as yet met between the covers of a single textbook. They are antithetical in purpose: the former is to teach the techniques, the latter, to limit the use of the techniques. Further, even if such a book were produced, it would be unlikely that it would receive a wide audience in contemporary political science. At the very least such a book would no doubt be regarded as inappropriate for this course, as it would create confusion.

<sup>28</sup>R. Golembiewski, W. Welsh and W. Crotty, Methodological Primer, p. 48.





<sup>29</sup>Ibid.

<sup>30</sup>Ibid., p. 14.

<sup>31</sup>Ibid., p. 30.

<sup>32</sup>Ibid.

<sup>33</sup>Ibid., p. 49.

<sup>34</sup>E. Dreyer and W. Rosenbaum, "The Study of Public Opinion and Electoral Behavior," Dreyer and Rosenbaum, eds., Political Opinion and Electoral Behavior (Belmont, Calif.: Wadsworth, 1966), pp. 1-11.

<sup>35</sup>Ibid., pp. 4-5.

<sup>36</sup>S. Ulmer, "Scientific Method and the Judicial Process," American Behavioral Scientist, VII (1963), p. 21.

<sup>37</sup>K. Deutsch and L. Rieslebach, "Recent Trends in Political Theory and Political Philosophy," Annals of the American Academy of the Political and the Social Sciences, CCCLX (1965), p. 139-140. Cf. J. Gunnell, "Social Science and Political Reality," Social Research, XXXV (1968), p. 164.

<sup>38</sup>p.2, N.2, above.

<sup>39</sup>E. Meehan, Theory and Method and Explanation.

<sup>40</sup>Meehan, Theory and Method, p. 37.

<sup>41</sup>Ibid., p. 39.

<sup>42</sup>W. Ebenstein, A Book Review of Political Theory, by A. Brecht, Annals of the American Academy of the Political and Social Sciences, CCCLVI (1959), p. 171.

<sup>43</sup>A. Brecht, Political Theory (Princeton University Press, 1959), p. vii.

<sup>44</sup>Ibid., p. 29-113.

<sup>45</sup>Ibid., p. 28-29.

<sup>46</sup>Ibid., p. 28.

<sup>47</sup>Ibid., p. 28-29.

<sup>48</sup>Ibid., pp. 29-113.



<sup>49</sup>A. Brecht, "Political Theory: Approaches," in D. Sills, editor, International Encyclopedia of the Social Sciences, XII, p. 308. Cf. Brecht, Political Theory, pp. 275-279 and 483-484.

<sup>50</sup>F. Frohock, The Nature of Political Inquiry (Homewood, Illinois: Dorsey Press, 1967), p. 110.

<sup>51</sup>Ibid.

<sup>52</sup>Ibid.

<sup>53</sup>Ibid., p. 111.

<sup>54</sup>H. McClosky, Political Inquiry (New York: Macmillan, 1969), p. 9.

<sup>55</sup>Ibid.

<sup>56</sup>H. Eulau, The Behavioral Persuasion in Politics (New York: Random House, 1963), p. 9. Cf. "...an empirical science is built by the slow, modest, piecemeal cumulation of relevant theories and data," Ibid., p. 114, and Eulau, "The Behavioral Movement in Political Science," Social Research, XXXV (1968), p. 28 and Eulau, "Tradition and Innovation," in Eulau, ed., Behavioralism in Political Science (New York: Atherton, 1969), p. 15.

<sup>57</sup>Eulau, "Segments of Political Science Most Susceptible to Behavioristic Treatment," J. Charlesworth, ed., Contemporary Political Analysis (New York: Free Press, 1967), p. 33.

<sup>58</sup>McClosky, Inquiry, p. 8.



### CHAPTER THREE

#### THE NOTION OF SCIENTIFIC METHOD HELD BY THE CRITICS OF THE IDEA OF A POLITICAL SCIENCE

I argued in Chapter Two that the adoption of a method of inquiry is important business. That argument need not be repeated here for the case that the rejection of a method is important is significantly different. The rejection of one method of inquiry does not have the same systematic, that is to say logical, effects on inquiry. Moreover, as opposed to mere neutrality or apathy toward a method, rejection is a direct challenge. As such its importance lies in the logical and popular strength of its arguments against the method in question. There are thousands of esoteric methods which arouse no movement of rejection because they are not powerful enough to challenge the assumptions or individual scientists or disciplines as a whole. Such rejection is generally aimed at methods widely perceived as dominant in one's own discipline. Methods which are not so perceived are avoidable and may be ignored without discomfort, but methods widely perceived as dominant cannot be avoided and ignored. They arouse their intellectual opponents for, by their nature, they begin to shape the whole discipline. The importance of such movements of rejection is two-fold. (1) The analysis and criticism of the movement of rejection may show genuine weaknesses in the method widely perceived as dominant and perhaps even suggest ways to improve the existing method or reasons for scrapping it. (2) If the analysis and criticism gain widespread support within the discipline they may depose the method, or may lead, at least, to a radical revision of that method.





In this chapter I shall try to characterize the understanding of scientific method, and in turn that of science, held by a range of opponents to political science qua science. Though such writers are no more numerous than the exponents considered in Chapter Two, their positions are not so unified as those of the exponents. The behavioral (scientific) movement in political science is unified by a concern for method, particularly, as we have seen, a method derived from the natural sciences. Critics of the idea of a political science do not have a comparable unifying motif, sharing only a common foe and not a common perspective or goal. For this reason critiques of political science qua science are exceedingly diffuse, and therefore, it is necessary to concentrate attention in this essay on a restricted range of critiques. Further, as with the exponents of the idea of a political science, many of the critiques are only vaguely formulated, as I shall note on occasion.

There have always been political scientists who have not been identified with, nor have identified themselves with, political science qua science. Such non-identification is, of course, not alone sign of critical opposition to the idea of a political science. The whole of political science is not exhausted by positions pro and con on behavioral (scientific) political science. Many, if not most, political scientists have been able to study politics without engaging in the essentially second order considerations of methodology presently under consideration.

Temporally, the first "resistance to the incorporation of scientific method has come in the form of an appeal to the past--to classical political science, such as natural law...."<sup>1</sup> The critiques mounted from this



traditional perspective have maintained that certain phenomena should not be analyzed, or treated scientifically.<sup>2</sup> Critiques of the idea of political science of this kind have been thoroughly and exhaustively analyzed by Kalleberg.<sup>3</sup> In this essay I will not give specific attention to such critiques. I choose to omit their consideration because of Kalleberg's exhaustive analysis of them (up to 1960) and on the grounds that their attack has failed to have any considerable effect on the discipline. However, a number of new critiques of the idea of political science have arisen which, judging from the vigorous debate they have already generated within the discipline, are of much greater impact.<sup>4</sup>

The critiques I will consider in this chapter belong to what has been called the "post-behavioral revolution," a term I shall adopt for convenience.<sup>5</sup> In contrast to traditional resistance, "(T)he post-behavioral revolution is...future oriented. It does not especially seek to return to some golden age of political research...."<sup>6</sup> In contrast to the traditional resistance which maintained that certain phenomena should not be analyzed scientifically, it holds "that some things cannot be analyzed" scientifically.<sup>7</sup>

In what follows I shall try to characterize the understanding of science held by political scientists whom I consider a part of the post-behavioral revolution, by examining their understanding of scientific method. Just as there is to date no full-blown philosophy of political science held by practitioners of behavioral political science, neither is there a well developed negative philosophy of political science held by critics of the idea of political science. Hence, within the restricted range of critics I will consider, a well-developed philosophy of science sort of argument will not be found. To my knowledge the critique





showing the most promise of movement in this direction is that of Wolin, thus, without meaning to imply that he is representative, I will single him out for special consideration. I do so because of the relatively thorough development of his critique and the widespread influence he has due to his reputation as a scholar.

Those writers considered are chosen on the basis of their having taken explicit and clear positions in criticism of the whole or a part of behavioral (scientific) political science.

The post-behavioral critiques of political science qua science are of three sorts. (1) "(T)he work of the behavioralists...is pervaded by implicit and unrecognized conservative values."<sup>8</sup> (2) "(T)he behavioralists have... shown that the concept of democracy developed by classical theorists...is not translated into practice in the major Western democracies...." They seem quite undisturbed and even happy in this knowledge.<sup>9</sup> (3) "(T)he behavioralists are termed 'anti-political'.... (T)hey have shown a marked tendency to throw politics out altogether."<sup>10</sup>

While critiques such as these are primarily ideological in character--in that they center on the alleged conservative, anti-democratic and anti-political ideologies of behavioral (scientific) political scientists--they are however, not entirely devoid of philosophical analysis into the nature of political science qua science. Clearly such philosophical arguments are a vital part of the post-behavioral critique. It would be one thing for the post-behavioralists to show that scientific political science is presently corrupt in some way. It would be quite another thing were the post-behavioralists to show that by its nature





scientific political science is condemned inherently to such corruption. It is to this latter point that philosophical arguments apply. Though the post-behavioral movement is not yet so mature as to have a well developed philosophical position it does most certainly have one, or rather, several related ones. In these pages I will restrict my attention to those parts of critiques speaking most directly to methodological, that is philosophy of political science, issues. I adopt, at the same time, two techniques to consider post-behavioral analyses of the philosophy of political science. My first technique is to consider arguments of three analytic sorts: (1) the misapplication of science, (2) the limitations of science and (3) the inappropriateness of science. Secondly, for each of the three sorts of arguments that I have set out I shall pay primary, but not exclusive attention to one writer. Needless to say, I shall not concentrate on one man alone within each sort of question, for post-behavioral positions are not yet well enough developed to merit such attention. I shall fail in this attempt only with the third sort of argument concerning appropriateness where I shall consider one writer exclusively. It is therefore wise to consider more than one writer within each sort of argument and more than three writers all told. On the other hand as post-behavioral positions are not yet well developed, much of their force within the discipline derives from the personalities adhering to them. Consequently an emphasis on individual writers may be helpful. In times of crisis when argumentative positions are not clear, understandably, well-known personalities are often taken for benchmarks in assessing the situation. Hence, for the misapplication of science, I shall consider particularly Christian Bay; for the limitations of science, I shall center my attention on Herbert Marcuse, and for the inappropriateness of science I shall concentrate on Sheldon Wolin.



For some post-behavioral critics the shortcoming of behavioral (scientific) political science lies not in its (scientific) method but rather in its subject matter.<sup>11</sup> Consider Bay who has attempted to redefine both politics and political science starting from a classical liberal view on the purpose of the state. For Bay the "only acceptable justification of a particular form of government...is that it serves to meet human needs better than other forms of government."<sup>12</sup> Bay's "basic commitment to human survival as the overriding objective of politics is taken directly from Hobbes, and so is...(h)is corollary assertion that sheer survival is not enough."<sup>13</sup> The basic position Bay holds "is a commitment to the sanctity of every human life, physical and personal; not only to its sheer preservation but to its freedom...to grow and develop according to inner propensities and potentialities."<sup>14</sup> This leads Bay to "define as political all activity aimed at improving or protecting conditions for the satisfaction of human needs... according to some universalistic scheme of priorities...."<sup>15</sup> In contrast to the political is the "pseudopolitical" which "refers to activity that resembles political activity but is exclusively concerned with either the alleviation of personal neuroses or with promoting private or private interest group advantage, deterred by no articulate or disinterested conception of what would be just or fair to other groups."<sup>16</sup> While politics and pseudopolitics are not fully mutually exclusive most contemporary democratic politics in Bay's view is pseudopolitical.<sup>17</sup>

If most politics in countries where political science is found is pseudopolitical it follows that most political science in those countries will have as its subject matter pseudopolitics, becoming therefore, in Bay's terms pseudopolitical science.<sup>18</sup> In one sense this is quite understandable. Where pseudopolitics is politics it becomes the

... system for ... ..

[The following section contains several paragraphs of extremely faint, illegible text, likely bleed-through from the reverse side of the page. The text is too light to transcribe accurately.]

... ..



subject matter of political science by habit and default. Moreover, much about behavior can be learned from the study of pseudopolitical behavior. In another sense, however, a pseudopolitical science is more troublesome in Bay's view. He objects to "the way findings are usually reported and interpreted." He objects to "the tendency in much of the behavioral literature to deal with the pseudopolitical aspects of behavior almost exclusively, and to imply that the prevalence of pseudopolitics is and always will be the natural or even the desirable state of affairs in a democracy."<sup>19</sup>

Bay, then, pleads "for more and better behavioral research."<sup>20</sup> He wishes to have political science "develop and apply psychological models of need priorities as a basis for research....," or put differently, "to develop a scientific study of basic human needs...."<sup>21</sup> Bay contrasts his projected study of politics with that of most contemporary behavioral political scientists who are of the view that "political inquiry can be pursued by much the same methods as natural science inquiry" applied to the pseudopolitical problems which the pseudopolitical reality fabricates.<sup>22</sup>

"In this context" Bay uses the terms "'science'" and "scientific method" in a broad sense to mean "all careful systematic inquiry and theory of sufficiently detached nature to produce insights that can be shared by people who differ in...value preferences." More particularly "'(B)ehavioral science' is...a...reference to the social sciences and psychology.....: in short to the science devoted to the empirical study of human behavior." Further, once achieved "(s)cience is, or should be, a tool for solving human problems...." But of course, says Bay, human problems are never just empirical and accordingly "(I)t is true that the





scientific method cannot help us understand everything...."23

Bay offers only the most general specifications of science and scientific method per se except in connecting them to empirical research. He is, however, much more specific about his idea of a political science. Without reservation or qualification he speaks at the same time of "The scientific method" and the "methods" of "natural science inquiry."24 It seems then, that as Bay has repeatedly emphasized, he is not opposed to political science qua science, but rather, that he is opposed to its domain as presently conceived. He wishes to preserve political science qua science and apply it to a new domain.

An analysis of the present state of political science seemingly comparable to Bay's in intent is Surkin's. In Surkin's judgment the current view of "the policy scientist" is that he can "avoid violating the canons of scientific method of recognizing" the fact-value distinction.25 Denying the fact-value distinction, Surkin suggests an "existential phenomenological" approach to the science of politics. He contends that the approach of an existential phenomenology to political reality establishes intersubjectivity, meaning "the fundamental interconnection between the external, objective world including other people and the internal, subjective world of consciousness."26 The purity of knowledge maintained by the fact-value distinction is meaningless, for knowledge will be useful to and used by dominant social institutions anyway.27 This blending of objective and subjective, facts and values, allows knowledge to be meaningful by allowing it to be socially purposive. A search for political knowledge that begins with a recognition of this fact can turn to existential phenomenology for a meaningful and significant blending of fact and value.



Surkin is most certainly not opposed to the idea of a political science per se. Rather, he is anxious to preserve it (note that his reference to the "canons of scientific method" is made without discussion or qualification), and indeed to extend its domain to cover the twilight between political facts and political values, if not political values as well.

Bay and Surkin challenge contemporary political science on substantive and not methodological grounds. For them the problem with behavioral (scientific) political science lies not so much in its efforts at and achievements of science or scientific method as in its concentration on an ill-chosen subject matter. Uniformly they suggest that scientific method could be applied--and applied more fruitfully--to a different subject matter. They seem to pass over the notion of scientific method rather quickly and with little or no analysis of it as held by their opponents. It seems then that were their own views about the domain of political science qua science heeded they would then wish to use this unproblematic notion of scientific method in the advocacy and exercise of their new science of politics.

One way to describe the post-behavioral critiques concerning the limitations of scientific method and science might be that they criticize science for what it does not do. Consider Schutz who argues that modern political science qua science should be based on "knowledge gained from feeling, seeing, caring, desiring, as a member of an affected human community."<sup>28</sup> To bring this interaction between knowledge and action to consciousness should be, he maintains, the task of social science. "All else is elaboration of it or instrumental aids of it."<sup>29</sup> Because modern political scientists are not political activists it is concluded that they





shirk this task. "The irrelevance of political science to politics results from the divorce of its practitioners from political action."<sup>30</sup> For Schutz "a law of political irrelevance might be...the more scientific the method, and the more amenable to the scientific method the subject matter, the less significant the findings."<sup>31</sup> But even to be both unscientific and a political activist are not alone sufficient. The "political action must itself be significant" it must not be "political escapism" into movements outside "the two-party system and its pressure-group complex."<sup>32</sup> Put differently, Schutz is claiming that contemporary political science qua science has a wrong notion of science; political science more properly involves the application of political knowledge through political action, for where else could the importance of knowledge lie except in its use? For Schutz contemporary scientific political science is irrelevant. He proposes a new, activist political science as science. Schutz does not take exception to the notion of scientific method itself. Rather, he is concerned with the implications of the application of scientific method.

Schutz outlines an anti-behavioral position on what might be seen as the limitations of scientific method in political science. Now the question becomes why is scientific method regarded by some as generically condemned? This brings us to an examination of Herbert Marcuse, for he has set out an argument on the nature of the limitations of scientific method and science. To Marcuse the present social "pattern of one-dimensional thought and behavior... (m)ay be related to a development in scientific method: operationalism in physical, behaviorism in the social sciences."<sup>33</sup> He sees behaviorism (read behavioralism) as a derivative of operationalism in that social science imitates natural science. Operationalism and behaviorism have "(t)he common feature" of "a total empiricism in the treatment of concepts; their





meaning is restricted to...particular operations and behavior."<sup>34</sup> Concepts are synonymous with the corresponding sets of operations or behaviors. In Marcuse's view Bridgman's prediction that "we shall no longer...use as tools... concepts which we cannot give an adequate account in terms of operations" has come all too true.<sup>35</sup>

In Marcuse's judgment this "new mode of thought is today the predominant tendency...."<sup>36</sup> As a result "(m)any of the most seriously troublesome concepts are being 'eliminated' by showing that no adequate account of them in terms of operations or behavior can be given."<sup>37</sup> Therefore, "the evolution of scientific method...'reflects' the transformation of nature into technical reality." This seems to mean that nature is then considered real only insofar as it conforms to "science and scientific thought with their internal concepts and their internal truth."<sup>38</sup> Accordingly "(p)ure science...retains its identity and validity apart from utilization."<sup>39</sup> Meanwhile applied science remains and, "(i)n view of the internal instrumentalist character of scientific method," there seems to be a close relationship "between scientific thought and its application--a relationship in which both move under the same logic and rationality of domination."<sup>40</sup> As a result, Marcuse contends, instrumentality itself is the only purpose or end of applied science, for all nature is defined by science and all science is defined by its method (operationalism or behaviorism).<sup>41</sup> The point is "that science, by virtue of its own method and concepts" dominates, not reveals, nature.<sup>42</sup>

Several other political scientists have contemplated the limits of scientific method and in turn of science. While their positions are not as full blown as Marcuse's they offer points that complement and supplement his perspective.



Sibley argues that "valuations enter into the choice of field and the subject for investigation." But, in his understanding, "once we descend" from these presuppositions "to the formulation of scientific hypothesis, the process of verification, it would appear, can in principle proceed without the value judgments coloring the observation." This is "to assert that methods exist in political as well as in natural science investigation to minimize the danger of such bias.... Within his nonscientifically derived framework, then, the behavioral scientist can be expected to provide us with an increasing body of scientific explanations and predictions."<sup>43</sup>

The question in Sibley's view is "whether scientific understandings, explanations and predictions are the only understandings, explanations and predictions...." "Are there," he asks, "things which the behaviorist approach cannot tell us about the world which the politician endeavors to understand?" In answering his rhetorical question Sibley maintains "that there are indeed several questions for which behavioralism cannot supply answers." (1) Behavioralism cannot provide scientific accounts of the behavior of the behaviorist as an observer. (2) Behavioralism cannot help tell us what we ought to value or to do in political life.<sup>44</sup> (3) Behavioralism cannot foretell the future for the predictions of science are of necessity specific and, even assuming that such predictions may be in some sense additive, they do not provide a general picture. "Questions of this kind cannot be answered scientifically."<sup>45</sup> Their solutions must "be sought in a combination of imaginative (and unscientific)" thought and experience.<sup>46</sup>

"This does not mean that...(Sibley) reject(s) behavioral science.... (It is) to suggest its limited role





in practical science and philosophy...." For "behavioral science takes us far out of the world of macrocosmic political 'reality' into the universe of pure scientific speculation..."<sup>47</sup> behavioral science "takes us away from the world in which men must make political judgments."<sup>48</sup>

Sibley shares Marcuse's view that a scientific political science would have us ignore the most important questions we face--and should be facing. Note that in this respect he contrasts scientific thinking with imaginative thinking. Thus Sibley wishes to see that the limitations of behavioral (scientific) political science are understood.

Cooper sees scientific inquiry to be based on the assumption that there are regularities in the subject matter's behavior. He holds that science in the search for or formulations of laws and in turn theories must (almost by definition) consist of generalizations. Clearly such generalizations can only be comprised of inquiries into regularities. Accordingly the irregular is disregarded for convenience.<sup>49</sup> But such disregarded irregularities are likely to become regarded, not merely as inconvenient but as unimportant. Indeed, insofar as some such irregularities disrupt or limit some generalizations, they may be regarded as illegitimate for, as Wolin has noted, the most visible regularities are those which are socially legitimate and those most visible irregularities are socially illegitimate.<sup>50</sup> Hence, there may be a simple transference. Generalizations, of course, requires abstraction. Generalization denotes dealing with the universal rather than the particular aspects of every member of a class, kind or group. Hence generalization connotes abstraction for abstraction expresses a quality, or aspect, apart from the whole object, or any specific instance of the object.





For Cooper such abstraction is a sign of the distance of the language of political science from politics. Abstraction may lend itself to a considerable conceptual elegance whose distance from its supposed domain is so great that all sense of prima facie connection is lost.<sup>51</sup>

Or as Kress argues,

The development of a specialized and technical vocabulary is properly viewed as one indicator of a mature science, but it is also a measure of the distance between the discipline and its materials. The language of contemporary political analysis reveals a deep conceptual hiatus between science and practice; political 'attitude' studies frequently employ categories of classification and reportage that have no meaning to the respondents they purport to describe.... their meaning is accessible to the former but not the latter.<sup>52</sup>

To Kress this is "'Platonizing' scientific concepts."<sup>53</sup>

For Cooper and Kress a scientific political science must inevitably part from the domain of politics. The gap between the two may increase, perhaps, until the former is quite meaningless in terms of the latter and vice versa. As the separation between the languages of political science and everyday politics increases the comfortably vague twilight zone between the two which has so long sheltered non-political scientific positions will disappear, or at least so Wolin contends as we shall see.

Both Sibley and Wolin have discussed the notion of tacit (implicit) knowledge first advanced by Polanyi.<sup>54</sup> Polanyi's notion of tacit knowledge, in Sibley's understanding, holds that there are two kinds of knowledge. First, there is explicit knowledge which is knowledge about groups of individual facts. Second, there is implicit



knowledge which is knowledge of the general nature of a thing, for example, of the pattern of some related facts and their relationship.<sup>55</sup> Explicit knowledge is "roughly equivalent" to "scientific theory and verification." Implicit knowledge is prescientific and post-scientific understanding. It "is a process of comprehending whereby we grasp parts and fit them into a whole."<sup>56</sup>

Implicit "knowledge is involved in the process both of communicating and of receiving communications....Because the symbols which we are compelled to use" in working with and communicating explicit knowledge "cannot be said to communicate an understanding of (the symbols) themselves...."<sup>57</sup> Rather, the comprehension of the individual addressed must be relied upon in part to grasp the meaning of the words in which the explicit knowledge is expressed. Therefore, "all knowledge is 'tainted' by personal participation at all levels. We cannot even begin to know and to understand without shaping that which is known."<sup>58</sup> For Sibley, after Polanyi, this does not invalidate knowledge but it does impair the sense of its objectivity.

For Sibley this notion of tacit knowledge defeats any attempt by the behavioralist to behaviorally, that is scientifically, study his own behavior as a behavioralist. This is one limitation that Sibley sees on behavioral political science.

Wolin carries Sibley's analysis further. It is his view that because "(p)olitical life does not yield its significance to terse hypothesis, but is elusive and hence meaningful statements about it often have to be allusive and tentative."<sup>59</sup> Context is, as a result, most important.<sup>60</sup> "Knowledge of this type tends, therefore, to be suggestive and illuminative rather than explicit and determinative."<sup>61</sup>





Such implicit knowledge is threatened in political science by the education in method political scientists receive. It leaves them no time and energy for non-methodical interests in politics and political science, and once achieved it is too big an investment of time and energy to be cast off and not used.<sup>62</sup> Method, and the explicit knowledge it is designed to yield, differs from implicit knowledge by being "impatient with the past" holding "that the truth of statements yielded by scientific methods has certain features such as rigor, precision, and quantifiability." Because the "connection between the statements and their features is" so intimate one can easily come to believe "that when he is offered statements rigorous, precise and quantifiable, he is in the presence of truth." Or put differently, if political science qua science is taken as equaling rigor, precision and quantifiability then rigor, precision and quantifiability may be taken as equaling science. On the other hand, attempted statements of fact that "palpably lack rigor, precision and quantifiability" are said to be false.<sup>63</sup>

In Wolin's judgment, implicit knowledge which he sees as necessary to the understanding of politics, "is being jeopardized" by this equation of truth with the product of method, and method itself, and contemporary behavioral (scientific) political science with methodism. In order to draw this conclusion Wolin may have reasoned something as follows. New explicit knowledge is always drawn from old implicit knowledge. Scientists seek explicit knowledge, in effect therefore, always trying to erode the domain of implicit knowledge. (This process is today sometimes termed, after Weber, the de-mystification of the world.<sup>64</sup>) Currently in political science this is done by methodists through their notion of method and the methodical education. It is Wolin's view that additional explicit knowledge will not only be at the expense of implicit



THE UNIVERSITY OF CHICAGO PRESS

knowledge but will also cause implicit knowledge to fall into disrepute and to be disregarded because explicit knowledge is much more highly and widely regarded than is implicit knowledge. In a reverse of Gresham's law, hard (explicit) knowledge drives out soft (implicit) knowledge. Presumably this is because hard knowledge can more easily be converted into the coin of the realm than implicit knowledge, that is to say that hard knowledge can be understood more easily and more widely than soft knowledge because it has those characteristics which we have seen exponents of the idea of a political science attribute to it.

At a most general level of concern then, those post-behavioral critics who have contended that science in the study of politics has certain inherent limitations have focused on the resulting insulation of political science from its domain, politics. Some have maintained that this insulation results in turn in the systematic disregard of certain kinds of political phenomena which come to be considered not only inconvenient, but illegitimate. Others in this group fear that scientific political science is unaware of its limitations and that its explicit political knowledge will erode the domain of implicit political knowledge and supplant it. Again, as with the critics of the misapplication of science, the notion of scientific method escapes careful analysis by this group.

The most recently formulated post-behavioral critique is that which in my view can best be described as challenging the appropriateness of the application of science and scientific method to the study of politics. This critique is thus far singularly the product of Wolin. As I have examined parts of Wolin's critique in part III above, it is coincidentally useful and fair that I look more closely at his



fuller argument in the pages which follow. A closer look at Wolin is particularly important because his is, in my reading, not only the most recent, but also the most developed post-behavioral critique. A good part of Wolin's argument is composed of a sociological analysis of contemporary scientific political science, behavioralism, and to a certain extent of politics itself.<sup>65</sup> This part of his argument is particularly acute, but falls outside the scope of my present concerns which are aspects of the philosophy and not the sociology of political science. While these two concerns are not exclusive of each other, each informing the other, in this essay I try to treat them as distinct, difficult though this may be.

Wolin declares that the main objective of "the study of politics is ... acquiring scientific knowledge about politics." Pursuit of this objective is now, he says, "dominated by the belief that" it "depends upon the adoption and refinement of specific techniques (methods) and that to be qualified or certified as a political scientist is tantamount to possessing prescribed techniques."<sup>66</sup> The techniques, or methods, are supposedly borrowed from the natural science.<sup>67</sup> "The study of politics has," therefore, "become increasingly scientific: that is, behavioral, quantitative, empirically oriented, experimental where possible, rigorous, and precise."<sup>68</sup> Reliable political knowledge is sought through the emulation of the "scientific procedures of observation, data-gathering, classification, and verification...." A traditional resistance to the adoption of "scientific methods" in political science proved inadequate and "it was increasingly assumed that the case had been proven for applying scientific methods to the study of politics...."<sup>69</sup>

The "advocates of science" in political science claim





two main benefits accrue from the extension of science into political science. It is their view that the application of the method of science will yield knowledge that is both precise and cumulative.<sup>70</sup>

In Wolin's judgment, the idea of method, or methodism, has certain inherent limitations. "Method," he writes, "is not a thing for all worlds.... The kind of world hospitable to method invites a search for these regularities that reflect the main patterns of behavior which society is seeking to promote and maintain."<sup>71</sup> Accordingly, the framework of assumptions of methodism is an "ideological paradigm reflective of the same political community" it is to study. "These assumptions are such as to reinforce an uncritical view of existing political structures and all that they imply."<sup>72</sup> The application of method assumes "that the truth of statements yielded by scientific methods has certain features, such as rigor, precision, and quantifiability. The connection between the statements and their features is intimate so that one is encouraged to believe that when he is offered statements, rigorous, precise and quantifiable, he is in the presence of truth."<sup>73</sup> Wolin's basic position is that "(a)ppropriateness of judgment cannot be encapsulated into a formula;" methodism assumes that it can.<sup>74</sup>

For Wolin then, political science qua science, including behavioral political science, equals methodism and methodism equals conservatism. In his words:

American political scientists continue to devote great energy to explaining how various agencies ingeniously work at political socialization of our citizens and new future citizens while mobs burn down parts of our cities, students defy campus rules and authorities, and a new generation questions the whole range of civic obligations. American political scientists have laboriously erected 'incrementalism' into a dogma and extolled its merits



as a style of decision-making that is 'realistic,' it is apparent to all that the society suffers from maladies -- the economic gap between our minorities and our majority, crises in the educational system, destruction of natural environment -- which call for the most precedent-shattering and radical measures.<sup>75</sup>

Like Cooper and Kress, Wolin judges political science to be isolated and insulated from politics -- its rightful domain.<sup>76</sup> This distance allows, "amidst" the "chaos" of the political world, "official political science to (exude) a complacency which almost beggars description."<sup>77</sup>

To come to terms with the hiatus of political science and politics today Wolin proposes that Kuhn's "new interpretation of science" be applied to the methodical political science of the hiatus and to the traditional (epic) political theory which preceded the hiatus.<sup>78</sup>

It is Wolin's view that "the nature of traditional (epic) political theory has been misunderstood...."<sup>79</sup> He argues that epic theory has shown the signs of the two main benefits advocates of a political science have seen in science: precise and cumulative knowledge. In epic theory "major theories have served as master-paradigms enabling later and lesser writers to exploit them....,"<sup>80</sup> just as in science there is an accumulation of knowledge derived from having first a few and then a number of scientists work on problems inspired by a common paradigm. He notes as illustration of this the remarkably lasting paradigm of Aristotle and its extensive use by a wide variety of people long after the demise of its creator. He compares the effect of the Aristotelian paradigm on the Middle Ages to that of Galileo, or Newton on their successors.<sup>81</sup>

In addition, epic theory may produce the benefit of

of the same kind as the other two, but it is not



precise knowledge, which also provides an empirical basis for the cumulation of knowledge. It is mistaken, contends Wolin, to suppose that epic theory was and is "'trans-empirical,' more concerned with transcending the world of facts than with formulating propositions which could be tested against the world of facts."<sup>82</sup> To be sure epic theory is skeptical of facts, but it is not ignorant of them.<sup>83</sup>

The contrast Wolin sees between epic theory and the methodism of behavioral (scientific) political science "is not between the true and the false (that is to say, propositions that are factual and hence may be shown to be either true or false and propositions which are not factual, but metaphysical, and cannot be shown to be either true or false)...but between truth which is economical, replicable, and easily packaged and truth which is not."<sup>84</sup> Methodism to the contrary, "(a)ppropriateness of judgment cannot be encapsulated into a formula."<sup>85</sup> For, the "sense of 'significance' (that is, the sense of what problems and questions are worth pursuing both socially and scientifically and in what way) which... is vital to scientific inquiry... cannot be furnished by scientific methods" anymore than new "theoretical vistas" can be opened by scientific methods.<sup>86</sup>

It seems, then, that for Wolin both epic theory and scientific political science have in common the same domain of study--politics, and the same product--scientific knowledge of politics.<sup>87</sup> Nevertheless, they differ significantly. Their difference is in the form of inquiry. Much of the energy of epic theory aims "at the creation of new paradigms."<sup>88</sup> It begins by questioning the first principles of politics, and the study of politics. This is the concept of the systematically mistaken and explains why most epic





political theories contain such radical critiques. "Their authors have tried to get at the basic principles which produce mistaken arrangements and wrong actions."<sup>89</sup> Methodists do not question such first principles at all, wishing only to get on with their application.<sup>90</sup>

Although Wolin in an earlier publication concludes that scientific political science and epic theory are not and need not be divorced<sup>91</sup> he offers no such conciliatory gesture at the close of his "Political Theory as a Vocation." Rather, here he leaves us with a clearly drawn contrast where the issue "is between those who would restrict the 'reach' of theory by dwelling on facts which are selected by...the...requisites of the existing (ideologically conservative) paradigm, and those who believe that...it is the task of the theoretical imagination to restate new possibilities."<sup>92</sup>

Wolin uses the term "scientific methods" occasionally without addressing the notion of scientific method as such.<sup>93</sup> In a sense he may be taking the notion and the techniques it implies as givens which are themselves unproblematic. Their use and effects, however, are quite problematic in his view. The spirit of methodism bred by scientific method has, in his judgment, corrupted political science. "(O)fficial political science" can no longer fulfill the main objective of any study of politics--the acquisition of scientific knowledge of politics. Its paradigm is conservative in nature. Its conservatism stems from the problems which methodism rules out of consideration (nonincremental ones, for example), and the way it treats problems it recognizes as problems (emphasizing regularity, for example). The political phenomena that exhibit characteristics of this kind, and are therefore susceptible to study by methodism, are those that



are socially dominant in the United States (and notions relatively) similar to them. This ideology isolates political science from politics, leaving political science basking in a misbegotten complacency. It can no longer aid in the compilation of scientific knowledge of politics. It can only distort the chaotic political world to fit its paradigm.

In order to progress in the compilation of scientific knowledge of politics Wolin insists that epic theory must be revitalized to turn aside the "triumph of methodism."<sup>94</sup>

In this chapter I have tried to characterize the understanding of science through the understanding of scientific method held by post-behavioral critics of political science qua science. Just as the exponents of political science qua science, as we have seen equate science with empirical research, so also do the post-behavioral critics. For them this equivalence is one of the givens of the notion of scientific method and the idea of a political science. By their general failure to challenge the notion of scientific method they seem to regard it as unproblematic. Its use is regarded as creating problems, but the legitimacy of the advocates' claims and assertions for it are largely unchallenged. Some post-behavioral critics wish to see science and scientific method applied to a new range of problems. Others hold that science and scientific method have inherent limitations which can only be ignored at too great a cost. Still others see science and scientific method as being ultimately inappropriate for the study of the political world. None of these critics challenge the legitimacy and significance of the notion of scientific method held by their opponents, which notions are the stimuli of their critiques. With the exception of Wolin, they seem as ready as their opponents to accept scientific





method as unproblematic in the natural sciences and unproblematically transferrable to political science. Wolin's exception runs thin for though this line of criticism is quite visible in "Paradigms and Political Theory" it is nearly invisible in "Political Theory as a Vocation."

After an examination of the idea of a science held by a number of philosophers of science (Chapter Four), I shall return to the views on science and scientific method held by the advocates and critics of the idea of a political science that I have just described (Chapters Two and Three) for further comment.



### FOOTNOTES, CHAPTER THREE

<sup>1</sup>D. Easton, "New Revolution," p. 1051.

<sup>2</sup>C. McCoy and J. Playford, "Introduction," McCoy and Playford, eds., Apolitical Politics (New York: Crowell, 1967), pp. 9-10.

<sup>3</sup>A. Kalleberg, The Idea of a Science of Politics.

<sup>4</sup>Eulau has said that the traditional resistences were purposefully ignored by behavioral political scientists because their tone was so intemperate. "Tradition," Eulau, ed., Behavioralism, p. 4. This is a collection devoted to the challenges and responses of the new critiques. A further illustration of the impact of the new critiques is the fact that the Presidential address of the American Political Science Association for 1969 consisted of a discussion of them; see Easton, "New Revolution."

<sup>5</sup>Easton, "New Revolution," p. 1051.

<sup>6</sup>Ibid., p. 1051.

<sup>7</sup>McCoy-Playford, "Introduction," p. 10. Emphasis added.

<sup>8</sup>McCoy-Playford, "Introduction," p. 3.

<sup>9</sup>Ibid., p. 6.

<sup>10</sup>Ibid., p. 7.

<sup>11</sup>Note Eulau's remark that early political behavioralism was distinguished not so much by its method, scientific or otherwise, as by its subject matter. In it the question asked was "How does the system work?" as opposed to the traditional legal normative question of "How is the system supposed (legally or morally) to work?"; Eulau, "Behavioral Movement," pp. 6 and 9.

<sup>12</sup>C. Bay, "Needs, Wants, Desires and Political Legitimacy," Canadian Journal of Political Science, 1 (1968), p. 241. Cf. Bay, Structure of Freedom (New York: Anthem, 1964), pp. xxiii and 3.

<sup>13</sup>C. Bay, "Political Legitimacy," p. 241.

<sup>14</sup>Ibid.



<sup>15</sup>Bay, "Politics and Pseudopolitics," American Political Science Review, LIX (1965), pp. 39-57; reprinted in McCoy-Playford, eds., Apolitical Politics, p. 15. Cf. Bay, "Liberalism," Centennial Review of Arts and Sciences, IV (1968), p. 213.

<sup>16</sup>Bay, "Pseudopolitics," p. 15. Cf. Bay, "The Cheerful Study of Dismal Politics," T. Roszak, ed., The Dissenting Academy (New York: Pantheon, 1968), p. 213.

<sup>17</sup>Bay, "Pseudopolitics," pp. 16 and 15.

<sup>18</sup>Ibid.

<sup>19</sup>Ibid.

<sup>20</sup>Bay, "Dismal Politics," p. 213. Cf. Bay, "Pseudopolitics," p. 13.

<sup>21</sup>Bay, "Dismal Politics," p. 213.

<sup>22</sup>Bay, "Pseudopolitics," p. 15.

<sup>23</sup>Bay, "Liberalism," pp. 343-344.

<sup>24</sup>Ibid.

<sup>25</sup>M. Surkin, "Sense and Nonsense in Politics," Political Science, II (1969), p. 579. Cf. A. Wolfe and M. Surkin, "The Political Dimension of American Political Science," Acta Politica, V (1970), pp. 43-61.

<sup>26</sup>Ibid., pp. 580-581.

<sup>27</sup>Ibid., pp. 579 and 575.

<sup>28</sup>A. Schutz, "Significance and Action in Social Science," Ethics, LXXIII (1963), p. 235.

<sup>29</sup>Ibid.

<sup>30</sup>Ibid., p. 234.

<sup>31</sup>Ibid., p. 233.

<sup>32</sup>Ibid., pp. 233-234.

<sup>33</sup>H. Marcuse, One-Dimensional Man, (Boston: Beacon Press, 1965) p. 12. In my view Marcuse's emphasis on logical positivism as the totality of science combined with the fact that his understanding of logical positivism stops short with





Bridgeman's developments of forty-two years ago exaggerating his case to a point where it is nearly irrelevant to the present situations. However, this view is not widely shared for Marcuse and his arguments are highly regarded by many political scientists.

<sup>34</sup>Ibid.

<sup>35</sup>Ibid., p. 13. Cf. P. Bridgeman, The Logic of Modern Physics (New York: MacMillan, 1928), p. 31.

<sup>36</sup>Marcuse, One-Dimensional, p. 13.

<sup>37</sup>Ibid.

<sup>38</sup>Ibid., p. 154.

<sup>39</sup>Ibid., p. 155.

<sup>40</sup>Ibid.

<sup>41</sup>Ibid., p. 156.

<sup>42</sup>Ibid., p. 161.

<sup>43</sup>M. Q. Sibley, "The Limitations of Behavioralism," J. Charlesworth, ed., Contemporary Political Analysis (New York: Free Press, 1967), p. 61.

<sup>44</sup>Ibid.

<sup>45</sup>Ibid., p. 66.

<sup>46</sup>Ibid.

<sup>47</sup>Ibid.

<sup>48</sup>Ibid., p. 67.

<sup>49</sup>Cooper, "Behavioralism, Pluralism, and Methodism," Paper Read At The Canadian Political Science Association Annual Meeting (1970), p. 27.

<sup>50</sup>Wolin, "Political Theory," p. 1064. For works applying this notion see C. Taylor, "Neutrality in Political Science," P. Laslett and W. Runciman, eds., Philosophy, Politics and Society, Third Series (Oxford: Blackwell's, 1965), pp. 25-57 and P. Kress, "Self, Society and Significance," Ethics, LXXVII (1966), p. 1-13.

<sup>51</sup>Cooper, "Behavioralism," p. 27.



<sup>52</sup>Kress, "Politics and Society," Polity, II (1969), p. 10. Cf. V. O. Key, Jr., "The Politically Relevant in Surveys," Public Opinion Quarterly, XXIV (1960), p. 54.

<sup>53</sup>Kress, "Politics," p. 10.

<sup>54</sup>M. Polanyi, Personal Knowledge (Chicago: University of Chicago Press, 1958).

<sup>55</sup>Sibley, "Behavioralism," p. 58.

<sup>56</sup>Ibid., p. 57.

<sup>57</sup>Ibid., p. 56.

<sup>58</sup>Ibid.

<sup>59</sup>Wolin, "Political Theory," p. 1070.

<sup>60</sup>See J. Gunnell, "Deduction, Explanation and Social Scientific Inquiry," American Political Science Review, LXIII (1969), pp. 1233-1246.

<sup>61</sup>Wolin, "Political Theory," p. 1070.

<sup>62</sup>Ibid., p. 1064 and 1071.

<sup>63</sup>Ibid., p. 1071.

<sup>64</sup>M. Weber, "Science as a Vocation," C. Mills and H. Gerth, eds., translators and introducers, From Max Weber (New York: Oxford University Press, 1946), p. 139.

<sup>65</sup>Wolin, "Paradigms and Political Theories," P. King and C. Parekh, eds., Politics and Experience (Cambridge: Cambridge University Press, 1968), pp. 149-152 and "Political Theory," p. 1064. For an interesting anticipation of Wolin see N. Jacobson, "The Unity of Political Theory," R. Young, ed., Approaches to the Study of Politics (Evanston, Ill.: Northwestern University Press, 1958), pp. 115-119. A more recent article by Spitz offers an analysis and remedy strikingly similar to that of Wolin (and Jacobson). Indeed it is so similar that it does not require separate treatment for the purposes of this essay. See D. Spitz, "Politics and the Critical Imagination," Review of Politics, XXXII (1970), pp. 419-435. See especially pp. 420, 428 and 430. For a thoughtful but unsystematic expression of this position see C. Hampden-Turner, Radical Man (Cambridge: Schenkman, 1970), pp. 1-12.

<sup>66</sup>Wolin, "Political Theory," p. 1063.





<sup>67</sup>Wolin, "Paradigms," p. 125.

<sup>68</sup>Wolin and J. Schaar, Review Essay: Essays in the Scientific Study of Politics, Edited by H. Storing, LVII (1963), p. 125.

<sup>69</sup>Ibid., pp. 125, 145 and 147.

<sup>70</sup>Wolin, "Paradigms," p. 125.

<sup>71</sup>Wolin, "Political Theory," p. 1064.

<sup>72</sup>Ibid.

<sup>73</sup>Ibid.

<sup>74</sup>Wolin, Letter to the Editor, American Political Science Review, LXIV (1970), p. 592. (He mistakenly cites his reference as p. 1077, the correct citation being p. 1076.)

<sup>75</sup>Wolin, "Political Theory," p. 1082. Cf. his Politics and Vision (Boston: Little Brown, 1960), pp. 359-363.

<sup>76</sup>See also H. Kariel, "Expanding the Political Present," American Political Science Review, LXIII (1969), pp. 768-776.

<sup>77</sup>Wolin, "Political Theory," p. 1081.

<sup>78</sup>Wolin, "Paradigms," p. 131. Emphasis added.

<sup>79</sup>Ibid., p. 126.

<sup>80</sup>Ibid., p. 141.

<sup>81</sup>Ibid., p. 142 and IV. 1.

<sup>82</sup>Ibid., p. 128.

<sup>83</sup>Wolin, "Political Theory," pp. 1081 and 1082.

<sup>84</sup>Ibid., p. 1071.

<sup>85</sup>Ibid., p. 1076.

<sup>86</sup>Ibid., p. 1077.

<sup>87</sup>Wolin, "Paradigms," p. 128 and Wolin, "Political Theory," p. 1063.

<sup>88</sup>Wolin, "Paradigms," p. 139.

<sup>89</sup>Wolin, "Political Theory," p. 1080.



<sup>90</sup>Wolin, "Paradigms," p. 125.

<sup>91</sup>Ibid., p. 152.

<sup>92</sup>Wolin, "Political Theory," p. 1082.

<sup>93</sup>See, e.g., "Paradigms," p. 125.

<sup>94</sup>Wolin, "Political Theory," p. 1077.



CHAPTER FOUR  
SCIENCE, SCIENTIFIC METHOD  
AND THE PHILOSOPHY OF SCIENCE

In this chapter I try to show that the notion of scientific method is not thought to be unproblematic by historians and philosophers of science. This is in contrast to the unproblematic way the notion of scientific method is held by both the advocates and the opponents of the idea of a political science.<sup>1</sup> One noted natural scientist has said,

I believe that almost all modern historians of the natural sciences would agree . . . there is no such thing as the scientific method.<sup>2</sup>

Conant's conclusion is, of course, interesting, but the arguments and logic that lead to it are of greater interest. It is to arguments of this sort that I shall now turn. Perhaps progress in this direction can best be made by first investigating the origin of the notion of scientific method.

A number of the writers I have examined to this point, have been concerned to point out that one of the distinguishing features of science is the aim of achieving explanations and theories that can be widely agreed to. This, it seems, is thought to have two requirements: (1) that the reasoning process be explicit, i.e. capable of being made public in full and (2) that the concept of what constitutes evidence be intersubjectively shared. Ideally, a witness, probably, but not necessarily another scientist, to a scientific theory should be able to feel that if he applied himself to the same problem he would come to the same conclusions and theory. It is important to note in this account that the key to both the witness's understanding of the scientist's theories and his agreement with or acceptance of it lies in the scientist's





doing his work in a series of steps. It is these sequential steps that provide the witness with a route by which he can penetrate the work of the first scientist so as to check on the validity and reliability of the work. The exercise of these steps reveals validity by virtue of showing that what it is claimed is being examined is in fact being examined. This is established by the intersubjective agreement between the scientist and the witness. Though it is not logically necessary that the witness be a scientist, in fact, he most likely is, for as we shall see later, the scientific community is the ultimate judge of a scientist's work qua science, both as to what it is the scientist claims to be examining and what constitutes an examination of it. Whether or not they agree is determined by whether or not they both come to the same conclusion after executing the steps of inquiry, i.e. steps of scientific method. Reliability checks are provided in the same way. The presence of the steps invite and allow the witness to replicate the scientist's work to see that his design has excluded no confounding independent variables. If a scientist's work is to be well received it is obviously necessary that other scientists find it acceptable. For them to do so, they must be confident that they might scrutinize the work and perform checks on its validity and reliability if they were so inclined. This can be most effectively and efficiently done by the provision of certain rules and steps which allow for the penetration and intersubjectivity of scientific work. As such, double checking is time consuming and the quantity of scientific output is great even for the normally proficient critique establishment of publishing houses, professional journals, university departments, research units, and conferences. One of the most important characteristics of these rules is that they draw parameters around what will be regarded as worth double-checking in the first instance. Only those



things which give the appearance of being fruitful in terms of the common understanding of science which these substantive and methodological parameters define will merit the attention of scientists.<sup>3</sup>

Notions of criteria for what will be checked and the criteria for what constitutes acceptable validity and reliability plainly are important. In effect, the notion of replication through procedures, provides both a self-imposed and group-imposed criterion governing what will be put forward as science. Like most of us, I believe more things to be true than those whose truth I can cogently argue for according to the standards of logic and evidence of a science. Perhaps inevitably, our opinions cover a bigger space, a larger reach of time, a greater number of things, than anyone of us can observe and understand. It seems to be the conclusion of the preceding view that such a theory, not both derived and explained by this method, has not earned the appellation of "science." It may well earn it later. This of course, is not ruled out, but it has not yet earned it.

It would be foolish to argue that this position is totally foreign to science for it is not. I am sure many of us can attest to this on the basis of our high school and college science work. This sort of notion of scientific method is part and parcel of the idea of science as it is taught at such levels. And, indeed, it is a part of science more generally, but only a part. It is the matter of what part it is, and what the other part or parts might be to which I shall now direct my attention.

No doubt we are all aware of stories detailing how this or that great scientific discovery came about, in some considerable part, by accident. There are a number of such





stories which vary in detail and no doubt, in accuracy as well. I suppose that one thinks quickly of Newton and the apple, and Fleming and the penicillan mold and Semmelweis and infection. The importance of such stories lies not in their empirical truth. Rather, it lies in the point they all go toward--namely that some of the progress of science occurs not as a result of the application of scientific method, but as a result of accident. Merriam recognized this many years ago, when he noted that,

. . . unexpected relations may be discovered while looking for others, as has happened again and again in the field of science. Thus the investigator who started to find one combination discovered another. We say 'by accident', but in reality, he stumbled upon another relation while in the line of scientific experiment. In like manner, the explorer may look scientifically for one thing, and the boldness and thoroughness of his method may unearth something quite unexpected.<sup>4</sup>

Unfortunately, but not unexpectedly, Merriam does no more than mention in passing that this is one source of interest and adventure in scientific work. The interesting point is that the results of such accidents can come to attain status as science at all. How can this be the case? Does it not violate the notion of scientific method and in turn, the idea of science? The answer to this question lies in the fact that the notion of scientific method is, as I suggested above, only a part of the idea of science and not, as it is often thought, the whole of it. In order to come to grips with my rhetorical question, let me try to place these thoughts in context.

In what follows I shall cite a good number of instrumentalists and use their arguments rather extensively. My interest in so doing is not to urge the adoption of a single epistemological position, but rather to show the importance of discovery in science. It happens that this thesis has been



argued strongly, but not exclusively, by instrumentalists.

In order to emphasize this point I must in part anticipate what follows. The notions of justification and discovery can be variously applied. They can be used to refer to the distinction between breakthrough and normal science. They can further be used to refer to two different kinds of thinking and activity that are present within normal science (what might be true; what can be shown to be true). In the course of a days work, the typical scientist surely engages in both kinds of thinking and activity and it might be very difficult to put a particular behavior item unambiguously under one heading. This should not bother us or lead us to doubt the distinction for, to this point, all that is claimed is that any informed view of science must include considerable emphasis on discovery.

But if justification is to be seen as part of science, as the very word suggests, we should be able to say where it is to be found (otherwise how will we be able to say that a particular justification has indeed been justified?--we need to have the justification in front of us, as it were, so that we may place it under rational scrutiny). In order to see how this can be done we must distinguish scientific process and scientific product. It can then be seen that the term justification in the requisite sense is best associated with product, particularly the rational reconstruction of the product: this is what can be subjected to the sort of evaluation necessary.

Discovery is part of the scientific process and can only with some violence be included in the rational reconstruction of science. Justification is part of science and can best be approached through the rational reconstruction of particular scientific justifications.

CONTENTS

ORIGINAL ARTICLES

THE PROBLEM OF THE PHYSICIAN IN THE CITY

THE PHYSICIAN AND THE PUBLIC

THE PHYSICIAN AND THE LAW

THE PHYSICIAN AND THE ECONOMY

THE PHYSICIAN AND THE FUTURE

THE PHYSICIAN AND THE PAST

THE PHYSICIAN AND THE PRESENT

THE PHYSICIAN AND THE PEOPLE

THE PHYSICIAN AND THE PROFESSION

THE PHYSICIAN AND THE PATIENT

THE PHYSICIAN AND THE PUBLIC

THE PHYSICIAN AND THE LAW

THE PHYSICIAN AND THE ECONOMY

THE PHYSICIAN AND THE FUTURE

THE PHYSICIAN AND THE PAST

THE PHYSICIAN AND THE PRESENT

THE PHYSICIAN AND THE PEOPLE

THE PHYSICIAN AND THE PROFESSION

THE PHYSICIAN AND THE PATIENT

THE PHYSICIAN AND THE PUBLIC

THE PHYSICIAN AND THE LAW

THE PHYSICIAN AND THE ECONOMY

THE PHYSICIAN AND THE FUTURE

THE PHYSICIAN AND THE PAST

THE PHYSICIAN AND THE PRESENT

THE PHYSICIAN AND THE PEOPLE

THE PHYSICIAN AND THE PROFESSION

THE PHYSICIAN AND THE PATIENT



Reichenbach provides us with a convenient analytic framework for this problem. He argues that science operates in two contexts. These are the context of discovery and the context of justification. Science is not unidimensional but rather (at least) two dimensional. The contexts are analytically separable. Of course the separation is more difficult empirically. Indeed, to a certain extent, it is even difficult in principle because there is such interpenetration between categories. This is because just as the contexts can be separated, they can also be unified by virtue of their each constituting a part of science. If this common characteristic unites them, what is it that separates them?

Reichenbach's point of departure is "the well-known difference between the thinker's way of finding his theorem and his way of presenting it before a public."<sup>5</sup>

Here Reichenbach is in part, referring to the sort of accident such as I have mentioned above. But his point is more purposive than that generally conveyed by the stories of such happy accidents. These stories are usually taken to describe extraordinary incidents of accidental discovery whereas Reichenbach's thesis is that the extraordinary is, in fact, the ordinary in scientific discovery. For Reichenbach, all scientific discoveries occur as a result of insight or whatever one wishes to call the subjective variable of the imagination and genius of the scientist. There are then two sorts of thinking in science for Reichenbach, "the form in which thinking processes are communicated to other persons" and "the form in which they are subjectively performed."<sup>6</sup> For example, I am sure that we have all thought of explanations of a phenomenon that we thought useful and then searched for arguments and evidence to support our conviction.<sup>7</sup> In the course of such searching, the explanation's credibility in our own eyes and in those of others



10000 10 50000 11 10000 12 10000 13 10000 14 10000 15 10000 16 10000 17 10000 18 10000 19 10000 20 10000 21 10000 22 10000 23 10000 24 10000 25 10000 26 10000 27 10000 28 10000 29 10000 30 10000 31 10000 32 10000 33 10000 34 10000 35 10000 36 10000 37 10000 38 10000 39 10000 40 10000 41 10000 42 10000 43 10000 44 10000 45 10000 46 10000 47 10000 48 10000 49 10000 50 10000 51 10000 52 10000 53 10000 54 10000 55 10000 56 10000 57 10000 58 10000 59 10000 60 10000 61 10000 62 10000 63 10000 64 10000 65 10000 66 10000 67 10000 68 10000 69 10000 70 10000 71 10000 72 10000 73 10000 74 10000 75 10000 76 10000 77 10000 78 10000 79 10000 80 10000 81 10000 82 10000 83 10000 84 10000 85 10000 86 10000 87 10000 88 10000 89 10000 90 10000 91 10000 92 10000 93 10000 94 10000 95 10000 96 10000 97 10000 98 10000 99 10000 100 10000

may be strengthened or weakened. Reichenbach's thesis is that we think in two sorts of ways. One way is to me best described as lateral. This is the imaginative and intuitive; it is undisciplined. It moves sidewise in analogies, metaphors, parallels and the like, rather than vertically by logic. It is bold and its reach does indeed exceed its grasp. By contrast, the other way of thinking is unimaginative and cautious. It moves in straight lines either deductively or inductively. For its reach and grasp are one. "(T)he terms context of discovery and context of justification mark this distinction," says Reichenbach.<sup>8</sup> Rules of argument and evidence only apply in the latter context of justification for it is only here that one attempts to argue or persuade. But, of course, the latter context obtains invariably with the former for if it did not, we would either have no knowledge of instances of the former, or we should not treat them seriously.

The intuitive element in the context of discovery is, of course, that which sets it off from the context of justification. Reichenbach, as we shall see, holds the view that the permeation of this element in discovery is limited. For Reichenbach, it springs from the fact that even "scientific language . . . (is) destined like the language of daily life . . . (to) contain so many abbreviations and silently tolerated inexactitudes that a logician will never be fully content with" it.<sup>9</sup>

In the ordinary use of these terms, Reichenbach's idea might be explained in the following way. Thinking in the context of discovery is unscientific in that the notion of scientific method is not applicable or operative. Thirteen years later, he wrote,

The art of discovery escapes logical analysis; there are no logical rules in terms of which a "discovery machine" could be constructed . . . <sup>10</sup>



The context of justification must always follow that of discovery if the discovery is to become credible as science. Thinking in the context of justification is science in the sense that the notion of scientific method is operative: there are in this context, applicable rules and procedures. (One of the great difficulties in the empirical application of the analytic distinction of context is that "abbreviations" always exist.) The context of justification is "the rational reconstruction of knowledge" and "belongs to the descriptive task of epistemology"<sup>11</sup> and thus by belonging to the realm of epistemology, the notion of rules or method and the element of control they are intended to introduce, is applicable. "(T)he function of scientific controls is to channel critiques and to facilitate rather than to generate discoveries by routine. Control provides, in short, no mechanical substitutes for ideas."<sup>12</sup> If the context of justification is constituted of such rules of logic and evidence, what are the characteristics of the context of discovery? We have seen why Reichenbach sees them to differ in principle; how do they differ in fact?

The context of justification conforms closely to the notion of scientific method, as I have discussed it thus far, particularly in this chapter and in Chapter Three. Further detail, therefore, can be of little use. The other part of science, discovery, requires more elaboration, so that it may be contrasted with the notion of scientific method or the context of justification held by both the advocates and the opponents of the idea of a political science treated in this essay.

The first characteristic of science in the context of discovery is that science cannot be exhaustively characterized logically. An exhaustive characterization would require the





consideration of the psychology and sociology of scientists and science. As we shall see later in greater detail, this impossibility of characterization results from the fact that there are no immutable criteria for what will count as a scientific discovery. What will be taken to count is up to the community of scientists and what will persuade them. In the context of discovery they must be persuaded that the discovery in question reveals something about the world which is true and would otherwise not be revealed. More importantly, in the context of justification, they must be persuaded according to the current rules of logic and standards of evidence. As is the case in any pursuit, at times the form, justification, of the activity becomes more important to many, perhaps most of a time, than the substance of the discovery. (It should be pointed out immediately that the channeling effect that common rules of justification have is in its own way advantageous for scientific progress. It directs the energies and resources of scientists at a limited number of problems as seen from a limited number of points of view, thus mobilizing and concentrating their energies.<sup>13</sup>) By examining the context of discovery, we can learn only what science has and has not been in the past. We cannot, in this way, learn what science is or can be. Science is protean. Attempts to characterize science in the context of discovery are futile and in their turn, so all efforts to characterize science must be. Kaufman is correct when he says "that a specific science . . . should" not "be defined in terms" of "propositions representing our knowledge at a given time."<sup>14</sup> Such propositions are durable, but neither immutable nor permanent. Kaufman, in my judgment, errs when he asserts as a corollary to this position that a specific science should be defined in terms of the rules of procedure. His error is two-fold (1) it overlooks the two different sorts of thinking that I have argued



constitute science and, (2) thereby defines only a part of science and not a whole. As a partial understanding it is correct. It is, however, not a complete definition. Yet, this is, of course, what the advocates and opponents of the idea of political science try to do.

Within the confines of this essay, three perspectives on the context of discovery and the notion of scientific method, are in my judgment, particularly worthy of consideration. They are (1) Popper's notions of conjectures and refutations, (2) Hanson's notion of visual gestalt and (3) Polanyi's notions of implicit and explicit knowledge. These three perspectives contrast as sharply as I could wish, science in the context of discovery from science in the context of justification (scientific method).

Popper has long been puzzled by the problem of determining when something is scientific and when it is not and how we know it either is or is not.<sup>15</sup> It is his conclusion that,

Scientific knowledge starts with, and progresses by, unjustified (and unjustifiable) anticipations . . . by conjectures. These conjectures are controlled by criticism; that is, by attempted refutations.<sup>16</sup>

Popper's reasoning begins by accepting Hume's conclusion that induction cannot be logically justified.<sup>17</sup> Hume held that there can be no valid logical argument allowing us to establish "that those instances, of which we have had no experience, resemble those, of which, we have had experience." Thus, "even after the observation of the frequent or constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience."<sup>18</sup>





In Popper's view, Hume failed to follow this insight to its logical conclusion.<sup>19</sup> Rather, Hume decided that repetitions in the world cause us to expect regularities (generalities). Popper turns the table on Hume,<sup>20</sup> "(i)nstead of explaining our propensity to expect regularities as the result of repetition as Hume has done, (Popper) proposed to explain repetition-for-us as the result of our propensity to expect regularities and to search for them."<sup>21</sup>

This allows Popper to replace Hume's psychological theory of induction with a thesis of conjectures and refutations which is best conveyed by his own words:

Without waiting, passively, for repetitions to impress or impose regularities upon us, we actively try to impose regularities upon the world. We try to discover similarities in it, and to interpret it in terms of laws invented by us. Without waiting for premises, we jump to conclusions. These may have to be discarded later should observations show that they are wrong.<sup>22</sup>

He continues that,

This was a theory of trial and error--of conjectures and refutations. It made it possible to understand why our attempts to force interpretations upon the world were logically prior to the observation of similarities. Since there were logical reasons behind this procedure, I thought that it would apply in the field of science also; that scientific theories were not the digest of observations, but that they were inventions--conjectures boldly put forward for trial, to be eliminated if they clashed with observations; with observations which were rarely accidental but as a rule undertaken with the definite intention of testing a theory by obtaining, if possible, a decisive refutation.<sup>23</sup>

For Hume, facts determined some of the shape of our inquiry, i.e. they lead us to expect repetitions. Contrary to this, Popper concludes that we have psychological disposition to formulate our inquiry in terms of a search for regularities. For Popper





this disposition precedes facts.

I do not believe that we ever make inductive generalizations in the sense that we start with observations and try to derive our theories from them. I believe that the prejudice that we proceed in this way is a kind of optical illusion, and that at no stage of scientific development do we begin without something in the nature of a theory, such as a hypothesis, or a prejudice, or a problem--often a technological one--which in someway guides our observations, and helps us to select from the innumerable objects of observations those which may be of interest.<sup>24</sup>

Further, science as understood by Popper is not a teleological, cumulative enterprise. The source of scientific creativity is not drawn out of that which is already known. Rather it involves breaking with the traditional ways of seeing things. It is not the seeing of new things in the old way, but rather seeing old things in a new way, which in turn allows for the seeing of new things.

It is Popper's belief that no scientific hypothesis is ever fully substantiated or proven. This leads him to argue that the activity of science aims at trying to falsify every hypothesis. Failure to achieve this with any hypothesis tends to corroborate it. Hence, the acceptance of a hypothesis is contingent not on its being proven in a positive sense but rather on its ability to withstand severe efforts at its disproof. This is a negative proof. Such corroboration represents a far weaker and contingent notion of proof than a positive notion offers.

Plainly this sort of imaginative thinking cannot be predicted on the basis of a set of rules or methods. It is eminently clear from this account of Popper's line of argument that the context of discovery is inherently a part of science. But the context of justification is not forgotten.

[The following text is extremely faint and largely illegible. It appears to be a multi-paragraph document, possibly a letter or a report, discussing various topics. The text is organized into several paragraphs, with some lines indented. Due to the low contrast and resolution, the specific words and sentences cannot be accurately transcribed.]

It occupies an important and perhaps crucial place in this scheme.

For we can say that it is irrelevant from the point of view of science whether we have obtained our theories by jumping to unwarranted conclusions or merely by stumbling over them (that is, by 'intuition'), or else by some inductive procedure. The question, "How did you first find your theory?" relates, as it were, to an entirely private matter, as opposed to the question, 'How did you test your theory?' which alone is scientifically relevant.<sup>25</sup>

Once a conjecture or refutation has been put forward, the right question is about its present, i.e. what does it tell us about the world and what does the world tell us about it. The wrong question is about the past, i.e. its origin. Clearly testing signals the introduction of the context of justification.<sup>26</sup>

Related to Popper's notion of conjectures and refutations is a thesis advanced by Hanson. It may be helpful to think of his contention as being a sort of visual gestalt. He writes,

Given the same world it might have been construed differently. We might have spoken of it, thought of it, perceived it differently.<sup>27</sup>

Perhaps Hanson's thesis can be given additional clarification by briefly recalling an old and well known illustration. What Hanson is describing in the above paragraph is exactly the same phenomenon manifested when we are shown an outline sketch that may be interpreted as portraying either the profiles of two faces or a vase. It may be construed as either with equal justification. Another drawing might as easily have three or four or more equally justifiable interpretations. Now, to Hanson, the multiplicity of acceptable interpretations applies to the very stuff of the world, not





just classroom examples. No true (meaning in this instance, as it often does, singular) interpretation of facts exists in the world awaiting our discovery. Rather, facts are interpretations which reside in the eye of the beholder. Continuing in Kant's tradition, and, in part, anticipating Hanson, Weber once noted, "'Culture' is a finite segment of the meaningless infinity of the world's process, a segment on which human beings confer meaning and significance."<sup>28</sup>

Toulmin reports that the discovery that light travels in straight lines was not therefore, the discovery that where previously nothing had been thought, in an ordinary sense, to be travelling, there turned out on closer inspection, to be something travelling--namely, light. Rather, he argues that,

The heart of all major discoveries in the physical sciences is the discovery of novel methods of representation, and so of fresh techniques by which inferences can be drawn and drawn in ways which fit the phenomena under investigation.<sup>29</sup>

Then, to Hanson, the creative scientist ". . . is not the man who sees and reports what all normal observers see and report, but the man who sees in familiar objects what no one else has seen before."<sup>30</sup>

For Hanson, scientific creativity does not involve a change in either the facts of the world, in our ways of knowing them, or in our knowledge of them. Rather, scientific creativity lies in the way in which we look at that with which we are familiar. By so doing, whole new patterns of interpretation may be generated. Occasionally, these patterns are far more useful than those favored at the time. On such occasions, they may then come to replace the older interpretation.

The argument upon the basis of which Hanson reaches this conclusion, runs something as follows. Beginning with

Page 1 of 1

The following information was obtained from the records of the Department of the Interior, Bureau of Land Management, for the year ending December 31, 1964.

The total number of acres of land owned by the United States is 1,000,000,000. The total number of acres of land owned by the State of California is 100,000,000. The total number of acres of land owned by the private sector is 900,000,000.

The following table shows the distribution of land ownership in California for the year ending December 31, 1964.

Category	Number of Acres
United States	1,000,000,000
State of California	100,000,000
Private Sector	900,000,000

The following table shows the distribution of land ownership in California for the year ending December 31, 1964.

Category	Number of Acres
United States	1,000,000,000
State of California	100,000,000
Private Sector	900,000,000

The following table shows the distribution of land ownership in California for the year ending December 31, 1964.

Category	Number of Acres
United States	1,000,000,000
State of California	100,000,000
Private Sector	900,000,000

The following table shows the distribution of land ownership in California for the year ending December 31, 1964.

Category	Number of Acres
United States	1,000,000,000
State of California	100,000,000
Private Sector	900,000,000

recent research on retinal sensations, Hanson is of the view that the important part of seeing is not the retinal picture so much as it is how this picture is intellectually interpreted. "People, not their eyes, see" he tells us.<sup>31</sup> Referring to illustrations such as that of the vase-faces sketch, Hanson claims,

These are different interpretations of what all observers see in common. Retinal reactions (to the figure) are virtually identical; so, too, are our visual sense-data, since our drawings of what we see will have the same content. There is no place in the seeing for these differences, so they must lie in the interpretations put on what we see.<sup>32</sup>

The crux of this argument then is that facts do not precede inquiry.<sup>33</sup> Claims such as Hanson's have established some credibility among linguists and psycholinguists. For instance, it is the Whorf<sup>34</sup> hypothesis that language determines a speaker's perception of the world around him. It seems that few today would support the extreme Whorfian view that language completely inhibits the expression of certain types of ideas for it has been shown that, with sufficient effort, it is possible to say anything in any language.<sup>35</sup> However, it has been shown that certain things are easier to say and, therefore, more likely to be said.<sup>36</sup> Organization of a sense experience is not itself seen as the lines of a drawing are. "It is not an element in the visual field, but rather the way in which elements are appreciated."<sup>37</sup> Thus,

A trained physicist could see one thing in figures 8: an X-ray tube viewed from the cathode. Would Sir Lawrence Bragg and an Eskimo baby see the same thing when looking at an X-ray tube? Yes, and no. Yes--they are visually aware of the same object. No--the ways in which they are visually aware are profoundly different. Seeing is not only the having of a visual experience; it is also the way in which the visual experience is had.<sup>38</sup>

In short, there is a sense of which seeing is a "theory-





laden" undertaking just as we have seen Popper say above. Observation of  $x$  is shaped by prior knowledge of  $x$ . Therefore, for example, our notions of significance and relevance depend on what we already know.<sup>39</sup>

It seems, then, that we can have no certainty that our perceptions and the conceptions upon which they rest correspond to the world. This is illustrated by Sherif's experiments with autokinetic phenomena. A revolving fan is illuminated by a focused light in an otherwise darkened room. The light flashes only at intervals. By altering intervals of the light flashes, the fan can be made to appear to turn faster, slower or to stop. An unwary empiricist, trusting to his eyes could easily lose a hand if he tested his belief and thrust it into the "unmoving" blades.

Compare this view of facts with that of Brecht, who is of the conviction that scientific method "supplies a type of knowledge that can be transmitted from any person who has such knowledge to any other person who does not have it, but who can grasp the meaning of the symbols (words, signs) used in communication."<sup>41</sup> Plainly Brecht's assertion becomes almost tautologous given Hanson's view that our expectations, which are formalized in our language, condition much of what we see. Thus if two people share a language, to a certain extent, they therefore share the beginnings of a common understanding of the world.<sup>42</sup> It is not surprising then that such people often, or generally, see the same things in the world. They are taught to do so by their very language. It would be more surprising if they did not, and this is, in some cases, the act of genius in scientific theory as seen by Hanson.

Hanson's conception of a fact is similar to that of Dewey. To Dewey a fact is a complex of sense data organized





with respect to a prior frame of reference. A person's past experience determines what he sees, gives it meaning, and on the basis of that meaning he reacts with an intellectual formulation of the event.<sup>43</sup> The point of this thesis can be put in the form of an illustration. Let us suppose that I am a science instructor of a particular subject like physics. Let us suppose, further, that on the first day of class, I ask "Will each of you take pencil and paper. Then will each of you please observe and write down everything you observe." I venture to guess that immediately upon hearing my instruction most, if not all, students in the class would ask, rightly, "Observe what?"<sup>44</sup>

Clearly, the moral of this story is that the "whatness" which we wish to observe is prior to and distinct from observation, and it shapes and colors our observations. It tells us where to look and what to look for. If our perception were not so guided and selected we would be so overwhelmed by a profuse confusion of sense phenomena that we would be quite likely short-circuited.<sup>45</sup> In short, "(i)n physics, it is no use even beginning to look at things until you know exactly what you are looking for: observation has to be strictly controlled by reference to some particular theoretical problem."<sup>46</sup>

These considerations are worth setting out in detail because they

can be used to illustrate an important fact. No competent scientist does pointless or unplanned experiments. There is no place in science for random observations, and only in the rarest cases have scientists made experiments whose results were of any value, without knowing very well what they were about. Before the scientist enters his laboratory at all, he must therefore have guidance about the kind of state of affairs worth investigation, the type of apparatus worth assembling, and the sort of measurements worth making.<sup>47</sup>



Or, as Toulmin has said elsewhere, "the questions we ask inevitably depend on prior theoretical considerations."<sup>48</sup>

Yet the simple admonition "observe" is a cornerstone of scientific method to both the advocates and the opponents of the idea of political science, one whose place in science neither group wishes to question. Plainly, on this view, progress in scientific knowledge is not cumulative in the sense of incremental. Instead it moves by leaps and bounds. Often, it progresses furthest when it breaks most sharply with the past.<sup>49</sup> Compare this view of scientific progress with those of some of the advocates whom I have already examined. Eulau, as we have seen, holds that "an empirical science is built by slow, modest and piecemeal cumulation of theory, methods and data,"<sup>50</sup> or "(a)n empirical discipline is built by the slow modest, piecemeal cumulation of relevant theories and data."<sup>51</sup> Like Eulau, McClosky holds that the "accelerating capacity" of "the natural sciences" is due in part to their achievement of "cumulative research."<sup>52</sup> The shared view of McClosky and Eulau is somewhat at variance with that of the views of creativity in science we have seen thus far.

The third and last thesis concerning scientific creativity I will discuss in this essay has been set forward in the greatest detail by Polanyi. It is his contention that there are two sorts of knowledge. He labels them as explicit and tacit (implicit). The latter is, in a sense, the source of the former. "If we call the first kind explicit knowledge, and the second tacit knowledge, we may say that we always know tacitly that we are holding our explicit knowledge to be true."<sup>53</sup>

For Polanyi explicit knowledge is knowledge that can be





articulated and communicated by the printed or spoken word. It is then, in a sense, verbal knowledge. Tacit (implicit) knowledge, on the other hand, is pre-verbal knowledge. It cannot be related verbally and is developed only through experience. Polanyi then claims that it is the tension between these two forms of knowledge that impels us to seek that which we can hold to be true. He writes,

We seek to clarify, verify, or lend precision to something said or experienced. We move away from a position that is felt to be somewhat problematic to another position which we find more satisfying and this is how we eventually come to hold a piece of knowledge to be true. 54

Polanyi then concludes that the tacit (implicit) dimension of knowledge means that,

All human knowledge is now seen to be shaped and sustained by the inarticulate mental faculties which we share with the animals. This view entails a decisive change in our ideal of knowledge. The participation of the knower in shaping his knowledge, which had hitherto been tolerated only as a flaw--a shortcoming to be eliminated from perfect knowledge--is now recognized as the true guide and master of our cognitive powers.<sup>55</sup>

At this point the resemblance between Polanyi's position here and that of Reichenbach should be noted. Reichenbach's notion of abbreviation is, I think, quite similar to Polanyi's notion of implicit knowledge; and the notion of implicit knowledge in the context of discovery provides the seed of conjectures which are made possible by the variety of interpretations of reality which can be advanced without fear of contradiction from the world. Or, as Polanyi himself writes,

though such statements will be made in a form which best induces an understanding of their messages, the sender of the message will always have to rely for the comprehension of his message on the intelligence of the person



addressed. Only by virtue of his act of comprehension, of this tacit contribution of his own, can the receiving person be said to acquire knowledge when he is presented with a statement.<sup>56</sup>

In closing this treatment of Polanyi one implication of his position to which I will be returning shortly should be noted. It is that science cannot be taught through textbooks alone. Rather, it must be done to be fully appreciated. It must be interiorized. The individual participates in the creation and formation of his own knowledge of science and cannot be just a passive receptor of a set of articulated and articulatable views. Neither facts nor methods precede inquiry, or any individual inquirer.

If there are no rules or methods in the context of discovery, how can we tell if someone is doing science? Surely there are some criteria by which we can safely say that some sorts of activities that seem to fit the unrigorous admission requirements of the context of discovery will never be capable of meeting the rigorous admission requirements of context of justification, and hence ought not to be thought of as belonging to the domain of science. While this is easy to do empirically at any one time, it is more difficult to do analytically by virtue of the fact that, as we have seen, the criteria for justification are a product of the community of contemporary science.<sup>57</sup> Just as the definition of the word "science" has changed in the past and may change again in the future, so, too, the criteria of the context of justification may also change.<sup>58</sup> Perhaps the only thing that can be said analytically, is that science is that which is regarded as science by scientists. This is, in Kuhn's terms, ordinary, normal science. The science of the context of discovery is in intent, revolutionary, abnormal science. A particular piece of research may be scientific by the criteria I have advanced and be only normal science.

1. LITERATURE ON VSMI ASSOCIATION TO CRIMINAL INSTABILITY A. ...



But a scientific discipline cannot be only this or it will stagnate.

Just as it is helpful to think of science as having two contexts, it is also helpful to think of science as being of two sorts, normal and abnormal.<sup>59</sup> Normal science is the enlargement of the context of justification when this context is dominating scientific work. During such normal times, work is directed at a certain set of widely known questions and gone at in a commonly approved fashion. It is a sort of a process akin to filling out a puzzle where the parameters of the domain and method of the work are given. The justification of a piece of such work to the scientific community is a necessary and sufficient condition for us to be able to say that science has been done in any particular case.

However, if science or a particular scientific discipline is to long endure, more than this is necessary. It is at this point that the context of discovery becomes important, for it is abnormal or revolutionary. In essence, discovery is, as we have seen, the casting off of the old set of questions and the givens they entail, and the conversion to a new set of questions. Again Weber, as though in anticipation, wrote, "A new 'science' emerges where new problems are pursued by new methods and truths are thereby discovered which open up new points of view."<sup>60</sup>

Discovery, or creativity, is a necessary part of science because it reveals new truths about the world that was outside the domain of the old questions while including all the information the old questions yielded. But, it is also inevitably part of the process of science as I have attempted to show in the foregoing analysis in this chapter. At the level of scientific communities discovery is a necessary condition.





It is clear that science, in the sense of the context of discovery, is not just a matter of hard work. It involves acts of genius and insight which cannot be produced purposively. Yet, there is a link between scientific work (or training) and creativity, or discovery. Insofar as scientific work and training expose one to both the explicit and implicit dimensions of science, Toulmin holds,

One cannot teach a man to be imaginative; but there are certain kinds of imagination which only a man with a particular training can exercise.<sup>61</sup>

In a similar vein, Weber once observed that,

Ideas occur to us when they please, not when it pleases us. The best ideas do indeed occur to one's mind in the way in which Ihering describes it: when smoking a cigar on the sofa; or as Helmholtz states of himself with scientific exactitude: when taking a walk on a slowly ascending street. . . . In any case, ideas come when we do not expect them, and not when we are brooding and searching at our desks. Yet ideas would certainly not come to mind had we not brooded at our desks and searched for answers with passionate devotion.<sup>62</sup>

Yet, "the scientific worker" must "take. . . the risk" that he may "never have" a "valuable idea of his own."<sup>63</sup>

But the more important question of linkages is: What is the link between the context of justification and the context of discovery? Justification and discovery are both parts of science.

"Scientific discoveries," as Toulmin tells us, "do not consist in arguments which are plausible ad hominem, but rather in explanations which will stand on their own feet" before one's scientific colleagues.<sup>64</sup> Discoveries may easily be plausible. But such discoveries that remain only as this are not scientific discoveries. Justification is the necessary and sufficient condition of science. Discovery is not necessary nor sufficient in any one particular instance. This means

1. The first part of the report deals with the general situation of the country and the progress of the work during the year. It is divided into two main sections: the first section deals with the general situation of the country and the progress of the work during the year, and the second section deals with the specific results of the work.

2. The second part of the report deals with the specific results of the work. It is divided into three main sections: the first section deals with the results of the work in the field of agriculture, the second section deals with the results of the work in the field of industry, and the third section deals with the results of the work in the field of commerce.

3. The third part of the report deals with the financial results of the work. It is divided into two main sections: the first section deals with the income of the work, and the second section deals with the expenditure of the work.

4. The fourth part of the report deals with the general conclusions of the work. It is divided into two main sections: the first section deals with the general conclusions of the work, and the second section deals with the specific conclusions of the work.

that if a theory can be justified then we are sure that it is a part of science. The claim that a discovery has been made is not such a guarantee. For a discovery to gain the status of science, it must first prove itself capable of persuading scientists that it is scientific by withstanding their tests. That is to say that it first must be justified. This is true at the level of this or that theory or law or concept.

For science as a whole, justification and discovery are individually necessary and jointly sufficient for its existence. What does make an activity "scientific" rather than "sporting" or "artistic?" In part, this question calls for a simple taxonomy of what sorts of ideas have been considered to be scientific by whom at what time. But there is a further question behind such a taxonomy. It is, "Why have those particular ideas been considered to be scientific? This question is--by implication--what makes a scientific idea successful or unsuccessful? That is to say, "What makes an idea acceptable and accepted as a scientific one?"<sup>65</sup> "there is no universal recipe for all science and all scientists, any more than there is for all cakes and all cooks."<sup>66</sup>

Yet, with this caution it may be asserted that some essential characteristics of science are the intention of the scientist: (1) to be able to articulate the results of his work to his colleagues about (2) phenomena widely agreed to be empirical and (3) that his articulations will not be systematically deniable and falsifiable on the basis of the observations of such phenomena. The only one of these points I would want to defend vary far is here, fourth and last: (4) the central characteristic of science is the goal of bringing order out of the palpable disorder that greets the sense when one observes some domain of interest in the world.





In the preceding three paragraphs I have made some strong statements that together may sound like a formula. It must be clear that they are not a formula nor intended as such. Rather they are together a directive meant to point in a direction.

Perhaps a concrete illustration will serve to close discussion of this point. The image "of scientists moving coolly, methodically, and unerringly, to the results they report" stems from "the etiquette that governs the writing of scientific papers" which is itself a product of the context of justification. "This etiquette," Merton feels, "requires them to be works of vast expurgation, stripping the complex events and behaviors that culminated in the report of everything except their cognitive substance."<sup>67</sup> Compare the lean, taut, almost laconic, nine-hundred-word article that appeared in Nature in April of 1953<sup>68</sup> with the tangled web of event reported in Watson's forty-thousand word account of the same discovery.<sup>69</sup> In the former, Watson marches straight through his data to his conclusions. In the latter, a range of interpersonal relations, mistakes and even dreams are reported as bearing on the discovery. The former work is of the context of justification while the latter is of the context of discovery.

If the notion of the context of discovery is as familiar to natural science and philosophy of science as I have attempted to show, how is it that it has escaped so completely the notice of advocates and opponents of the idea of a political science whose scholarship is above doubt? I have argued that the two contexts are both parts of science. But this equality of status between these two contexts is not always fostered by the impact of science curricula. There, by omission or commission, the emphasis, it seems, more often



than not, is on the context of justification with only scant attention to the context of discovery. This assertion is confirmed by the casual eyeball empiricism of a bookstore browser. As a convenient example, the textbooks for undergraduate introductory courses available in the University of Alberta bookstore reveal this emphasis.<sup>70</sup>

Scientific textbooks perform the important task of putting science into a widely palatable form and serving it to a wide audience in lower level science courses. "(T)extbooks are the sole source of most people's firsthand acquaintance with the physical sciences."<sup>71</sup> I do not wish to belittle the importance of this task, yet, because it seems that science textbooks influence the image of science held by some natural scientists and most social scientists, the limitations of the textbook image of science require examination.

As teaching devices, science textbooks certainly must and do codify and unify science. While this view may be a moderately acceptable image of science to present to laymen, those who can least afford to hold this view are those who claim to be scientists. This is a particularly troublesome problem in view of Kuhn's assertion that "(t)he single most striking feature" of the education of a scientist "is that, to an extent totally unknown in other creative fields, it is conducted entirely through textbooks."<sup>72</sup> In the course of codifying and unifying science, textbooks must invariably conceal as much as they reveal. In so doing, that which is revealed are those aspects of science which give it coherence and unity; those concealed, those which give it incoherence and disunity. This is done simply as a convenience to teaching.

By this codification and unification, textbooks seem





inevitably to portray science as cumulative.<sup>73</sup> This sort of teleological imputation is understandable in that it represents an example of man's age-old habit of rewriting history to accord with his present understanding of the world.

Partly by selection and partly by distortion, the scientists of earlier ages are implicitly represented as having worked upon the same set of fixed problems and in accordance with the same set of fixed canons that the most recent. . . scientific theory and method have made seem scientific.<sup>74</sup>

Merkel when lamenting the difficulty in arriving at a definition of "political behavior" holds that some part of the difficulty arises "in part from the inclination of successful revolutionaries to reinterpret history as their own antecedent, turning the important figures of earlier political research into their precursors."<sup>75</sup> Treating science in this fashion tends to obscure the imaginative, speculative or creative side of it. This seems to be true for two reasons. First, those earlier scientists whose views cannot be reconciled with the written history of science are more or less totally ignored. Second, those scientists whose views were untraditional and imaginative are portrayed as commonplace and inescapably logical and are thus co-opted right into the written history.<sup>76</sup> Hence, textbooks present, in a manner of speaking, only one side of an argument, the winning side. Further, "(n)o textbook ever included a table that either intended or managed to infirm the theory the text was written to describe."<sup>77</sup> "Perhaps these conclusions are not surprising," Kuhn writes.

Textbooks are, after all, written some time after the discoveries and confirmation procedures whose outcomes they record. Furthermore, they are written for purposes of pedagogy. The objective of a textbook is to provide the reader in the most economical and easily assimilable form, with a statement of what the contemporary community believes it knows . . . <sup>78</sup>

Goodman has summarized the effect of textbook science





in this way, "The student is encouraged to discover the results which the present state of science knows beforehand."<sup>79</sup>

At the risk of oversimplification, it might be said that textbooks imply that there are unchanging truths regarding both substance and method which the student of science can learn and that once he has learned them, he knows science. This sort of certitude is not scientific.<sup>80</sup> There are not unchanging answers. There are true answers. While such answers may be regarded as true they can also be fruitfully re-examined and changed, which could not be the case if they were in an ordinary sense true. They cannot be learned with more than limited benefit. On the one hand justification is most important and it can be effectively taught. On the other hand discovery in science cannot be taught for it has no rules. It can only be shown to exist by encouraging creativity.

Textbooks convey explicit knowledge and thus facilitate its transmission and comprehension. As we have seen, Toulmin and Weber have said that explicit knowledge is the father to implicit knowledge. It sets one's thinking to a subject. Implicit knowledge is not derived from explicit knowledge in terms of content, only in terms of mental discipline and concentration. "The fact is that scientific investigation, as distinct from the theoretical content of any given branch of science, is a practical art. It is not learnt out of books, but by imitation and experience."<sup>81</sup>

The explicit dimension acts as a filter which screens out phenomena that, in the reasoned opinion of the scientific community, are not relevant to the field. It is within these parameters that implicit knowledge is attained. It is attainable because no explicit science can fully and consistently



express the world. This leaves room for the tacit dimension and the development of new (revolutionary) theories. In physics, for example, the center of interest at any time depends on the current background of ideas. These provide the standard of what is normal and of what is to be expected. This is the paradigm.

The convergence of thought that textbooks and paradigms exhibit and enforce in science is not to be thought of only as a disbenefit. This convergence does not exist by accident. The discipline these two phenomena bring to science in terms of uniting and concentrating efforts on a limited range of problems from a certain limited point of view brings great strength to science. In some part it explains the remarkable success of science in finding truths and ways to understand the world. It is this discipline that gives science much of its power.<sup>82</sup> For instance,

Today's scientist would be criticized as having chosen an inappropriate approach toward solving the problem of explaining the origin of infantile paralysis if he collected statistical data concerning the relation between variations of temperature and the incidence of this disease because he wanted material in support of the hypothesis that infantile paralysis is caused by fluctuations in temperature. To be sure, by doing so, the scientist has not violated the basic procedural rules of his science. He has not yet made the unfounded assertion that infantile paralysis is produced by temperature fluctuations, but has merely suggested that it is worth while to examine this hypothesis by collecting pertinent statistical material. However, at the present state of knowledge, it is generally believed that such data would be of no avail for the explanation of epidemics of infantile paralysis. Accordingly we can say that the gathering of data concerning temperature variations is presumably irrelevant to the explanation of the origin of infantile paralysis.

For any given problem, we can distinguish between chains of steps that are presumably relevant and chains of steps that are presumably irrelevant to its solution. The criteria of presumable relevance are preference rules of scientific procedure.<sup>83</sup>





(Kaufman's phrase "preference rules of scientific procedure" may be read as equivalent to the term "paradigm" which I employ.)

The explicit dimension of an individual scientist's paradigm then represents the tradition of his discipline as his teachers and contemporaries have come to understand and interpret it. Textbooks in science provide powerful teaching devices which save the student from a lengthy process of trial and error. They suffer at the same time, by concealing the status of the context of discovery and the implicit dimension of science.

This fact is important because textbooks are almost the singular teaching device in science. As we have seen, students of science learn about science by reading science textbooks, not by doing science. This view of science, from outside as it were, is of course, even more true of political science. Even the advanced student of science is forced to rely on textbooks until his third or fourth year of graduate study and is rarely exposed, Kuhn holds, to "the creative scientific literature" that made the textbook possible.<sup>84</sup> Students so taught, of course, teach their students in the same fashion, furthering the division of the contexts of discovery and justification which are empirically so close together. At best it can be assumed that political scientists, whether pro or con on the idea of a political science, have a knowledge of science based on textbooks.

In this chapter, I have argued that the notion of scientific method is problematic. This is the case because a part of science in both process and product is creative. This part I have called after Reichenbach, the context of discovery. It is inextricably a part of science and it is most unmethodical or nonmethodical. This context of discovery



is distinguished from the context of justification by virtue of its being unexplicit and abbreviated, whereas the latter context is insistently explicit and detailed. Three theses concerning creativity in science were examined: Popper's notions of conjectures and refutations, Hanson's notion of visual gestalt and Polanyi's notions of implicit and explicit knowledge. These arguments, with Reichenbach's notion of the inevitability of abbreviations, highlight (1) the inevitability of "creativity" both in providing science a subject matter, and, more importantly, in the process of science itself and (2) the considerable difference between the context of discovery and the context of justification. The link between the contexts, I have argued, rests in the necessity for a "discovery" to be justified if it is to be regarded as scientific. The question: How is it that the role of creativity in science has been neglected by political scientists concerned with the idea of a political science, as they were discussed in Chapters Two and Three? was considered. Here the concepts of scientific paradigms and textbook science were considered. In a double-barreled argument, I have contended that (1) paradigms bring a unity and coherence to science which is most particularly illustrated by science textbooks and the image of science which they convey, while disregarding nearly completely the role of creativity in science, and (2) it can be inferred from an examination of their writings that most practitioners of political science have had no deeper contact with science than such textbooks. This latter point is borne out, of course, by the similarity of their notions of scientific method to those conveyed by science textbooks. It is understandable that scientific paradigms and textbooks are as they are. It is unfortunate, however, that such practitioners of political science who are concerned with the idea of a political science should have only this textbook image of scientific method. This image of science is, it seems, regarded as the whole of science.



While textbook or normal science of the context of justification constitutes most of science by any spatial or temporal measure, it is not the whole of science.

Next I shall compare and contrast the three perspectives on the notion of scientific method and the idea of a (political) science (Chapter Five) I have set out: that of the advocates of a political science qua science (Chapter Two), that of the critics of a political science qua science (Chapter Three) and that of some philosophers and historians of science (Chapter Four).





#### FOOTNOTES, CHAPTER FOUR

<sup>1</sup>However it should be noted that some philosophers of science are prepared to advocate the unlimited application of scientific method much as we have seen Eulau do, see H. Mehlberg, "The Range and Limits of the Scientific Method," Journal of Philosophy, LI (1954), p. 285.

<sup>2</sup>J. Conant, Modern Science and Modern Man (New York: Columbia University Press, 1952), p. 43.

<sup>3</sup>See A. DeGrazia, "The Politics of Science and Dr. Verlitosky," American Behavioral Scientist, VII (1963), pp. 45-50 and 53-68; M. Polanyi, "The Republic of Science," Minerva, I (1960), pp. 54-73; M. Polanyi, "The Growth of Science in Society," Minerva, V (1967), pp. 533-545; M. Polanyi, "The Potential Theory of Absorption: Authority in Science has Its Uses and Its Dangers," Science, CXLI (1963), pp. 1010-1013; T. Kuhn, "Historical Structure of Scientific Discovery," Science, CXXXVI (1962), pp. 760-769 and B. Barber, "Resistance by Scientists to Scientific Discovery," Science CXXXIV (1961), pp. 596-602.

<sup>4</sup>C. Merriam, New Aspects of Politics, (2nd ed.; Chicago: University of Chicago Press, 1931), p. 6.

<sup>5</sup>H. Reichenbach, Experience and Prediction (Chicago: University of Chicago Press, 1938), p. 6.

<sup>6</sup>Ibid.

<sup>7</sup>See T. Kuhn, "The Function of Measurement in Modern Physical Science," Isis, LII (1961), pp. 161-163. Cf. R. Carnap, Philosophical Foundations of Physics, edited by M. Gardner. (New York: Basic Books, 1966), p. 245.

<sup>8</sup>Reichenbach, Experience, p. 7.

<sup>9</sup>Ibid.

<sup>10</sup>H. Reichenbach, The Rise of Scientific Philosophy (Los Angeles and Berkeley: University of California Press, 1966), p. 231.

<sup>11</sup>Reichenbach, Experience, p. 7.

<sup>12</sup>I. Scheffler, Science and Subjectivity (Indianapolis: The Bobbs-Merrill Company, 1967), p. 2.



<sup>13</sup>Kuhn has discussed the benefits of this "convergence" throughout his work, cf. for example, his "The Essential Tension: Tradition and Innovation in Scientific Research," in C. W. Taylor and F. Barron, editors, Scientific Creativity (New York: Wiley, 1964), pp. 341-354.

<sup>14</sup>A. Kaufman, Methodology of the Social Sciences (New York: The Humanities Press, 1958), p. 42.

<sup>15</sup>K. Popper, Conjectures and Refutations (London: Routledge and Kegan Paul, 1963).

<sup>16</sup>K. Popper, Logic of Scientific Discovery, (London: Hutchinson, 1959), p. vii.

<sup>17</sup>Popper, Conjectures, p. 42.

<sup>18</sup>D. Hume, Treatise on Human Nature, two volumes (London: J. M. Dent & Sons, Ltd., 1817), pp. vi and xii.

<sup>19</sup>Popper, Conjectures, p. 45.

<sup>20</sup>Or alternatively it might be thought that in so doing Popper is in fact returning us to Kant who asserted that "our intelligence does not draw its laws from nature but imposes its laws on nature," Popper, Conjectures, p. 48 and 191 and cf. C. Churchman, Predictions and Optimal Decisions (Englewood Cliffs: Prentice Hall, 1961), p. 235.

<sup>21</sup>Popper, Conjectures, p. 46.

<sup>22</sup>Ibid.

<sup>23</sup>Ibid.

<sup>24</sup>Karl Popper, The Poverty of Historicism, second edition (Routledge and Kegan Paul, 1961), p. 134-135.

<sup>25</sup>Ibid., p. 135.

<sup>26</sup>Some of the advocates of the idea of a political science mention the place of imagination in science, to be sure. Generally speaking this admission is quite brief and uttered it seems, as an act of duty with neither enthusiasm nor detail; see, for example, A. Isaak, Scope and Method, p. 149, J. Pennock and D. Smith, Political Science, p. 11, and V. VanDyke, Political Science, p. 186. However, it should be noted that R. Dahl in his "Behavioral Approach," p. 25, seems to have the enthusiasm if not the detail. He, at least, does not seem embarrassed by the necessity of such an acknowledgement.





<sup>27</sup>N. Hanson, Patterns of Discovery: An Inquiry Into the Conceptual Foundations of Science, (Cambridge: Cambridge University Press, 1965), p. 36.

<sup>28</sup>M. Weber, The Methodology of the Social Sciences, translated and edited by E. Shils and H. Finch with a foreword by E. Shils (Glencoe, Illinois: The Free Press, 1949), p. 81.

<sup>29</sup>S. Toulmin, The Philosophy of Science (Cambridge: Cambridge University Press, 1961), pp. 20 and 34.

<sup>30</sup>Hanson, Patterns, p. 36. Cf. Popper, Conjectures, pp. 184 and 187.

<sup>31</sup>Ibid., p. 6.

<sup>32</sup>Ibid.

<sup>33</sup>A similar view of reality has long flourished in the school of thought called the sociology of knowledge. Perhaps the most well known of its exponents is, of course, K. Mannheim in his Ideology and Utopia, translated by L. Wirth and E. Shils (New York: Harcourt, 1936 (1929)). A facile contemporary treatment of this school of thought is P. Berger and T. Luckman, The Social Construction of Reality, (Garden City, New York: Doubleday, 1966).

<sup>34</sup>J. Carroll, editor, Language, Thought and Reality, (New York: Wiley, 1956). See also R. Poppe, "'The Time Has Come' (The Walrus Said)," The Western Canadian Journal of Anthropology, I (1970), pp. 12-34.

<sup>35</sup>J. J. Gumperez, "Language and Communication," The Annals of the American Academy of Political and Social Science, CCCLVVIII (1967), p. 225.

<sup>36</sup>Cf. R. Brown and E. Lenneberg, "A Study in Language and Cognition," Journal of Abnormal and Social Psychology IL (1954), pp. 454-462.

<sup>37</sup>Hanson, Patterns, p. 13.

<sup>38</sup>Ibid., p. 15.

<sup>39</sup>Ibid., p. 19 and 20.

<sup>40</sup>M. Sherif, "Group Influence Upon the Formation of Norms and Attitudes," in T. W. Newcomb and E. Harley, editors, Readings in Social Psychology (New York: Holt, 1947), pp. 77-89.



<sup>41</sup>A. Brecht, Theory p. 114.

<sup>42</sup>Again see Gumperez, "Language;" Carroll, editor, Language; and Hanson, Patterns.

<sup>43</sup>J. Dewey, Logic: The Theory of Inquiry (New York: Holt, 1938), p. 128.

<sup>44</sup>Popper offers this illustration in Conjectures, p. 46; cf. Popper, Discovery, pp. 106-111.

<sup>45</sup>See, e.g., C. Hempel, Philosophy of Natural Science (Englewood Cliffs, New Jersey: Prentice Hall, 1966), p. 13 and Popper, Conjectures, p. 118.

<sup>46</sup>Toulmin, Philosophy, p. 46.

<sup>47</sup>Ibid., p. 66.

<sup>48</sup>Toulmin, Foresight, p. 101.

<sup>49</sup>Kuhn, The Structure of Scientific Revolutions (2nd ed.; Chicago: University of Chicago Press, 1969), passim.

<sup>50</sup>H. Eulau, Behavioral Persuasion, p. 114.

<sup>51</sup>Ibid., p. 9, cf. also Eulau, "Tradition," in Eulau, editor, Behavioralism, p. 15, and Eulau, "Behavioral Movement," p. 28.

<sup>52</sup>H. McClosky, Inquiry, p. 8.

<sup>53</sup>Polanyi, The Study of Man (London: Routledge and Kegan Paul, 1958), p. 12. For a thorough critique of Polanyi's position, see A. Grunbaum, Philosophical Problems of Space and Time (New York: Knopf, 1963), pp. 377-386.

<sup>54</sup>Polanyi, Man, p. 26.

<sup>55</sup>Ibid.

<sup>56</sup>Ibid.

<sup>57</sup>See, e.g. Kuhn, Scientific Revolutions; Polanyi, "Republic;" Barber, "Resistance;" and DeGrazia, "The Politics of Science."

<sup>58</sup>See, e.g. Kuhn, Scientific Revolutions.

<sup>59</sup>See, e.g. Kuhn, Scientific Revolutions.

<sup>60</sup>Weber, Methodology, p. 68.



<sup>61</sup>Toulmin, Philosophy, p. 44.

<sup>62</sup>Weber, "Science," p. 136.

<sup>63</sup>Ibid.

<sup>64</sup>S. Toulmin, Foresight, p. 57.

<sup>65</sup>Cf. Toulmin, Foresight, p. 14.

<sup>66</sup>Ibid., p. 15.

<sup>67</sup>R. Merton, "Behavior Patterns of Scientists," The American Scholar (1969), p. 199.

<sup>68</sup>J. Watson and F. Crick, "Molecular Structure of Nucleic Acids: A Structure for Deoxyribuse Nucleic Acid," Nature, CLXXI (1953), pp. 737-738.

<sup>69</sup>J. Watson, The Double Helix (New York: Atheneum, 1968).

<sup>70</sup>See T. Ashford, The Physical Sciences, (2nd ed., New York: Holt, 1967), and P. Weisz, The Science of Biology (New York: McGraw-Hill, 1959).

<sup>71</sup>Kuhn, "Measurement," p. 163.

<sup>72</sup>Kuhn, "Essential Tension," p. 344; cf. Kuhn, Scientific Revolutions, p. 165.

<sup>73</sup>Kuhn, Scientific Revolutions, p. 138.

<sup>74</sup>Ibid.

<sup>75</sup>Merk1, "'Behavioristic' Tendencies in American Political Science," in H. Eulau, ed., Behavioralism in Political Science (New York: Atherton Press, 1969), p. 142.

<sup>76</sup>Ibid.

<sup>77</sup>Kuhn, "Measurement," p. 164.

<sup>78</sup>Ibid., p. 167.

<sup>79</sup>P. Goodman, "Mass Education in Science," J. Adams Lecture at the University of California at Los Angeles, 19 April 1966, p. 5.

<sup>80</sup>Though even an historian of science as sensitive to this problem as Kuhn, approaches this error in his own writing





as when he writes, "'energy is conserved: nature behaves that way'" quoted by C. Boyer in his "Commentary on the Paper of T. S. Kuhn," in M. Clagett, ed., Critical Problems in the History of Science (Madison: University of Wisconsin Press, 1959), p. 385.

<sup>81</sup>John Ziman, Public Knowledge: An Essay Concerning the Social Dimension of Science (Cambridge: Cambridge University Press, 1968), p. 7.

<sup>82</sup>See Kuhn, Scientific Revolutions, p. 28 and passim; and "Essential Tension," p. 342 and passim. Cf. N. Campbell, What Is Science (London: Methan, 1921); Ziman, Public Knowledge; W. Hagstorm, The Scientific Community (New York: Basic Books, 1965) and N. Storer, The Social System of Science (New York: Holt, 1966).

<sup>83</sup>Kaufman, Methodology, p. 70-71.

<sup>84</sup>Kuhn, Scientific Revolutions, p. 165.



CHAPTER FIVE  
POLITICAL SCIENCE, POLITICAL SCIENTISTS  
AND THE  
IDEA OF A POLITICAL SCIENCE

In this chapter I shall compare and contrast perspectives on the notion of a scientific method of the kinds discussed in the preceding chapters. I shall try to show that perspectives of the kinds described in Chapters Two and Three are, on several counts, inadequate and misleading in light of the perspectives described in Chapter Four. Were I to claim that those perspectives are entirely without merit, or were I to present a positive alternative of my own to them, a more exhaustive analysis would be in order. As my aims are as limited, a more selective analysis will be employed. I shall proceed by considering four main points that have either appeared or are quite germane throughout those three chapters; (1) the notions of implicit and explicit knowledge, (2) the notion of steps of inquiry, or scientific method proper, (3) the relationship of science and philosophy of science and (4) the factual basis of science. On the basis of the analysis of these four points I will, then, argue that the debate between the exponents and critics of science and scientific method in political science is, contrary to the apparent views of the antagonists, a pseudo-debate, for neither scientific method nor science as most properly conceived are at stake.

The notions of tacit (implicit) and explicit knowledge have appeared several times thus far in this essay. As we have seen, writers such as Sibley, Wolin and Polanyi have discussed them quite directly, while Eulau and McClosky have done so in a more general way.<sup>1</sup> In addition, Brecht, one of whose books I have treated above, has, elsewhere, commented specifically on the notion of implicit knowledge.





According to Brecht,

Polanyi...argues impressively that any type of knowledge even scientific, is ultimately based on personal 'belief' or 'commitment,' has failed to pay attention to the difference between nontransmissible 'decisions to accept' and the transmissible material to which they refer; even so, he does not seem to deny the heuristic advantages of generally recognized standards of science.<sup>2</sup>

Brecht is both correct and incorrect. Where he is correct, in my judgment, he does not say anything different from Polanyi - namely that the products of communities of scientists, just as the norms of any social group, are the product of that group. Where he is incorrect, Brecht fails to notice that for Polanyi the material, as it is seen by one person, is an experience that it is often difficult to convey to another person itself, apart from the additional complications of decisions about its assessment and use.<sup>3</sup> It is both the (criteria of) acceptance and the seeing of the "material" that according to Polanyi, can be transmitted only with difficulty. This is the "participation of the knower in shaping his knowledge."<sup>4</sup> Hanson's views on visual gestalt support Polanyi's thesis. Perception is, itself, problematic.

Nevertheless, Brecht continues,

The rebuttal that scientific arguments too are in need of ... 'belief' or 'commitment' does not wipe out the qualitative differences in transmissibility that severs evaluative from scientific arguments.<sup>5</sup>

But surely neither Polanyi nor any of the other philosophers of science from whom we have heard would deny that there is a difference between scientific argument and evaluative argument. Such a denial is neither the logic nor the intention of their arguments. Rather, it is Polanyi's view in this case and their view in general that the difference is



ultimately one of degree and not of kind. This is the logic of their positions. It is my view that it is particularly incumbent upon those who dabble in the philosophy of science, however delicately, as those writers whom I have treated in Chapters Two and Three have done, to be cognizant that the difference may logically be one of degree and not kind.

Among the post-behavioralists Wolin and Sibley have, as we have seen, specifically discussed this notion of implicit knowledge. It is Wolin's view that the tension between implicit and explicit knowledge in political science must inevitably favor explicit knowledge. For Wolin each gain in explicit political knowledge is a loss for implicit political knowledge, and in time implicit political knowledge will either entirely disappear or be disregarded.<sup>6</sup> In light of Chapter Four it can now be seen that while there is indeed a tension between implicit and explicit knowledge, -- an everpresent tension, science strives for explicit knowledge, as it operates in the context of justification, that this tension cannot logically be resolved in favor of explicit knowledge, Wolin to the contrary. This is the case for the implicit dimension of knowledge will always remain, at least, in undefined words relied on in communication. These are the abbreviations of which Reichenbach has spoken.<sup>7</sup> They remain each time an undefined term is defined. Each defined term is defined by undefined terms, i.e. terms whose meaning is understood prima facie requiring no definition themselves to be understood in that context. Such terms inevitably remain. For each time an undefined term is defined it must be defined by undefined words. Logically there are no defined words with which we may define our first terms. Indeed, this is impossible for what would they be defined with? The comprehension of those parts of propositions using such undefined words must, as Reichenbach and Polanyi have each





noted, be entrusted to the receiver.<sup>8</sup> Further, science has two contexts: justification and discovery. In the context of justification the coin of the realm is indeed explicit knowledge, though some residual implicit knowledge is logically inescapable; it is characteristic of the context of justification that every effort be made to minimize, according to the standard shared by practitioners of the particular discipline, the domain of implicit knowledge. However, in the context of discovery which is inescapably a part of science, as I have maintained, explicit knowledge is not logically possible. For explicit knowledge to be possible it is necessary to have knowledge of the subject matter in question. This is exactly what is in question in the context of discovery -- the development of knowledge which did not exist previously. Such new knowledge cannot be developed explicitly, for if it could be it would not be new knowledge; it would be deduction from existing knowledge. Once new knowledge is devised it can be elaborated and justified in the context of justification, the process of normal science. The development of knowledge (in the context of discovery) requires decisions. Such decisions, about knowledge, cannot be made on the basis of knowledge for that is exactly what is in question. Hence the decisions must be based on guesswork. Such guesswork is based in large part on experience and its implicit knowledge and, inevitably, in part on chance. The rationale for such decisions cannot be conveyed explicitly for that would require knowledge about knowledge.

Now, as we have seen, it is Wolin's view that science in political science has, in a most arbitrary fashion, attached itself to an exceedingly narrow conception of the domain of politics, a conception that, in Wolin's view excludes most of politics.<sup>9</sup> Wolin's contention is that political science qua science inevitably sees politics only in terms of





stability and incremental change, giving it an ideological conservative bias. Another post-behavioral critic has considered the same charge. Consider:

The indictment that behavioralism is in some sense conservative because science tends to be incremental in its procedures has some merit to it. If one thinks of science as a body of procedural rules some weight must necessarily be attached to the charge. For in this sense, defined as a series of procedures and rules, science is intrinsically conservative. The process of operationalizing, quantifying, and cautioning against excessive generalizing operate to inhibit the phrasing of research questions in a grand, speculative manner.<sup>10</sup>

In other words, science can be thought of as necessarily conservative only if it is thought of "as a body of procedural rules." As I detailed in Chapter Four the idea of a science is not exhausted, by any means, by such a view. Though this view does bear on science in the context of justification, we have seen that science has analytically two contexts, the second being that of discovery. In the context of discovery the notion of procedural rules according to which inquiry proceeds is quite inapplicable. If this is the case, then the notion that a political science is necessarily conservative in its social impact cannot stand. If science itself is not necessarily incremental, then, in all likelihood, neither will its effect on society be so. The political flavor of whatever impact a part of science has on society is a question of fact and not of logic. It is most certainly conceivable that a scientific change which was revolutionary within its own disciplinary terms of reference might have only a conservative impact on society in terms of political ideologies. By the same token scientific findings produced through the most painstaking and incremental steps may have a revolutionary impact on society.

More precisely, because the first principles of science are only defensible dialectically as Wolin notes, "science



retains an inherent flexibility." For "so long as those commitments retain an element of the arbitrary, the very nature of normal research ensures that novelty shall not be suppressed for very long" as "there are always discrepancies" between a paradigm, or a scientific theory, and their domains. In the course of research these discrepancies, called anomalies, will continually show up and conjectures and refutations will be proposed to account for them.<sup>12</sup> Both because the first principles of science are defensible only dialectically, and because science in the context of discovery is not logically fully explicable, there are no absolute rules about the nature of science itself. As a result, challenging theories and established theories share to that extent, the same unsure footing. Conjectures and refutations, of course depend on new conceptualizations of sense phenomena, such as we have seen Hanson describe.<sup>13</sup> Conceptualization of that sort thrives upon and in implicit knowledge. And, of course all of these elements are combined in the context of discovery. Brecht mistakenly takes Polanyi to be contending that science is somewhat less worthy an endeavor because of its implicit dimension. So too Wolin seems to take, wrongly in my judgment, the dialectical character of science as a sign its failure to be science.<sup>14</sup> However, science need not fail for it can change at its very roots -- its first principles. A scientific discipline does not stand or fall on any particular part of its first principles so much as it does on its successes and failures in finding truths and conceiving of useful understandings of its domain of sense phenomena.

Some of the exponents of science in political science have discussed the notion of explicitness in science, notably McClosky.<sup>15</sup> He takes the view that science requires precision, explicitness, rigorousness and operationalism. McClosky is not wrong in what he says. But what he says does not apply to





science generically, though by failing to specify exceptions he seems to mean it to. His views apply to science only in the context of discovery. For as we have seen such ideas make no sense there. Scientific inquiry cannot be conducted according to a set of rules. Consequently, McClosky, though he seems to think otherwise, is talking about only a part of science and not the whole of it.

While it is by now clear that there is a common sense core to the notion of scientific method, it should be equally clear that it is only a core. In view of the discussion in Chapter Four it can be seen that the notion applies to science only in the context of justification. It is here that communities of science develop rules -- rules of justification -- to be applied to scientific work post factum to determine whether such work meets certain currently accepted standards and receives fairly widespread approval from scientists who have worked in the same area. If a piece of scientific work can stand up to this scrutiny in the context of justification it will be regarded as scientific and then provide an acceptable basis for future work. The rules of justification are often mistaken for rules of scientific inquiry by outsiders observing science and, as we shall see, by some scientists as well; these rules arise out of the consensus judgment of each particular scientific community. Often these rules are seen as more important than the substance of science. "It is a strange science indeed which establishes as the ultimate test of theoretical worth the rigor with which a methodological formulation is defended rather than the significance of the hypothesis advanced...."<sup>16</sup> Once a piece of work has withstood the initial requirements of scientific justification it becomes a candidate for the convergence of the resources of the scientific community within which it arose and also communities with related domains. If a piece of scientific work cannot pass the



test of justification, further work on it is left to the proclivities of individual scientists and the turn of events. An idea which has once failed justification may, after further work or time, successfully be justified, for over time the requirements of justification may have changed or the idea may be resubmitted for a second attempt at justification due to the developments of further work.

Consequently, when Frohock,<sup>17</sup> for example, speaks of the steps of inquiry, or Eulau<sup>18</sup> speaks of asking questions which are known to be answerable in principle, as the key to scientific inquiry, they are mistaken. Inquiry cannot be conducted by known techniques and procedures. Inquiry is, as I have maintained above, the investigation into the unknown and hence our techniques and procedures, which are based on the known are, unsurprisingly, quite useless. They can serve the formulation only of the kinds of knowledge that went into their own creation, and this is not what is sought in the scientific inquiry of the context of discovery.

So too, when Wolin bemoans the lack of creativity in political science<sup>19</sup> he must realize that the perceived absence of creativity is a difficult assertion to support for: (1) How can creativity, as I have characterized it, be recognized, or alternatively how can Wolin characterize it? (2) How much creativity is enough and how much isn't enough? (3) How can even its absence imply its elimination for, as we have seen, it may always occur given the flexibility of science? and (4) In light of the disjunction bridged by creativity between the known and the unknown, how can creativity be either encouraged or stifled?

Neither is science tied to facts as Wolin implies.<sup>20</sup> As we have seen from Hanson, facts are not immanent awaiting recognition and hence they do not restrict our vision,<sup>21</sup>





though our conception of what constitutes a fact does. This conception is, of course, rooted in those arbitrary first principles of each particular scientific community. Scientific creativity involves a break with the past, a break which is no securer in its first principles than that dominant theory or paradigm it challenges. As can be seen, Wolin<sup>22</sup> is not unaware of the thoughts of Hanson, Popper, Polanyi, and especially Kuhn. He cites them on occasion and often seems to be using aspects of their thoughts to think about political science, just as I am.<sup>23</sup> The difference between us is that Wolin ends up with science called by another name. He uses Hanson, Popper, Polanyi and Kuhn to criticize his perception of political science qua science. He, then, uses their notions to suggest an alternative to political science qua science, epic theory. I contend merely, that the epic political theory he ends with has the characteristics of science as understood by the philosophers of science I considered in Chapter Four.<sup>24</sup> Wolin particularly emphasizes those aspects of science involved in the context of discovery. Contrary to his apparent intentions, Wolin is not proposing an alternative to science in political science, but simply a relabeling of the activity widely called science. At most, Wolin is reminding us that science involves both the rigors of the context of justification and the speculations of the context of discovery. Given the views of the advocates such as we saw in Chapter Two, this reminder may be useful. Though he seems to take behavioral political science, as it is presently constituted, as scientific political science and to offer his notion of epic political theory as an alternative to it, an unscientific alternative. He need not do this to press his views on political inquiry. The tradition within which, say Kuhn, whom he cites most frequently of all, writes is not, as Wolin supposes, new; it is, rather old. Indeed, in a review of Kuhn's book Gillespie writes that "it is not clear... that anyone holds the view of science which Kuhn would demolish."<sup>2</sup>





The same may be said of Wolin. He might have, therefore, set out his views on creativity in science -- of conjectures and refutations, perceptions and tacit knowledge -- not as unscientific or non-scientific as opposed to scientific behavioral political science but rather as more properly a part of a concept of science than those notions of science held by behavioral political scientists. In this way he might have enlisted in his own behalf the supposed allies of the advocates, the natural scientists and philosophers of science. This is not to deny that Wolin's view of scientific political inquiry is quite different from that of advocates of the idea of a political science, for it surely is. But, as we have seen, their views in turn are quite different from those of philosophers of science. Wolin's view is closer to that of the philosophers than of the advocates, one exception being the name they give to the activity they each characterize so similarly.

If this is the case then Wolin's notion suffers not only from being misnamed but also from concentrating on only one of the two contexts of science. Just as with the advocates of political science qua science, so too with Wolin, all science is not exhausted by one context, whether that of justification or discovery.

In 1963 Wolin (with Schaar) lamented that no philosophical critique of the new political science had yet been produced.<sup>26</sup> I am wondering if we must not continue the same lament after considering Wolin's latest efforts, for his critique of scientific political science is, by his own definition and logic, ideological. He claims that contemporary scientific political science is conservative, this being its failing.<sup>27</sup> He then proceeds to attack it. One of two inferences may be drawn from this. Either Wolin may be assuming



that being conservative makes scientific political science, in fact, non-scientific. This cannot stand for, as we have seen from Toulmin, the parentage or offspring of a scientific conjecture (or refutation), i.e. a scientific theory, is not relevant to its status as science.<sup>28</sup> Moreover, as we have seen from Polanyi, some scientific ideas do not have explicit origins let alone origins of a certain ideological character. A scientific idea must stand or fall on its own merits, not its lineage. Alternatively, Wolin may see being scientific as being conservative. In which case, if he is to offer an alternative, his position must first be non-conservative, or anti-conservative, and then non-scientific. His position, then, would, like that of his opponents, be distinguished primarily by its ideological and not its philosophical character. Accordingly, no genuine philosophical critique of the new political science can be intended by Wolin.

Wolin, among other post-behavioral critics, seems quite disturbed by the comments of advocates of the new political science on the nature of scientific and nonscientific political inquiry. It is, apparently, in this vein that he quotes Eulau's comments on the place of the prescriptive and the descriptive in political inquiry,<sup>29</sup> Riker on political science and political science and political wisdom,<sup>30</sup> Pool on normative and empirical theory<sup>31</sup> or Berelson and Steiner on the use to which the history of political science might be put.<sup>32</sup> Political scientists such as Eulau are lead no doubt in some part to their pronouncements on the philosophy of political science simply by the absence of a philosophy of political science worked out by philosophers of political science. Thus when Eulau is faced with philosophy of science sorts of questions either in his teaching or research, or in responding to critical challenges, he is left to his own devices, far outside his own area of specialty and interest. This problem is compounded by the fact that neither he nor his opponents, such as Wolin, seem to realize that he, Eulau, is outside his realm of competence.





How is it that Eulau may be outside his sphere of competence when he pronounces on philosophy of political science-relevant concerns? Suppose that all scientists denied the paradigmatic basis of their work. Let us further suppose that Wolin and I and all philosophers of political science agreed that the scientists were mistaken in this judgement. Does this allow us to infer, as Wolin would seemingly have us do, anything about the work of such scientists as scientists? I am inclined to think not. Scientists are not philosophers of science and vice versa.

As I mentioned in passing some time ago, any activity has two sides. In a physical activity like tennis playing, a practical one like nursing, or an intellectual one like theoretical physics the aspect seen by the observer differs importantly from that which engrosses the performer,

Only the practitioners can understand the training and practice, discipline and method, strategy and imagination called for in the supreme execution of his activity. Yet, at the same time, he may be so close to the activity that its most general features and widest connections begin to escape him.<sup>33</sup>

In an intellectual activity, such as science, or epic political theory, the knowledge derived from the domain commands the attention of the practitioners, not the nature of how the knowledge was derived. Hobbes, cunning though he was, believed that man knows what he makes.<sup>34</sup> The fact is that not even scientists are always adequately acquainted with their own work, the methods by which they do it, or its historical root and social setting. This is regrettable but not astonishing, for knowing about knowledge is not the regular business of scientists, unless they also happen to be philosophers of science. This is rightly so because each undertaking requires special techniques and has a particular domain. The domain of any science is the material of that part of the world with



which it is concerned. The language of science consists of propositions about the world. The domain of the philosophy of science is the activity of science. The language of philosophy of science consists of propositions about the activity of science.

Unlike the various sciences which have as their subject matter physical or biological or social phenomena, philosophy of science does not presume to add to our factual knowledge of the 'real world' but aims rather at increasing a critical understanding of the body of knowledge which the former are providing. Philosophy of science deals not with the subject matter of the various sciences but with the statements the sciences make about their respective subject matters; e.g. the ways these statements are determined, tested, explained and justified.<sup>35</sup>

Skills in the pursuit of one sort of inquiry do not transfer automatically to the other, even if an effort is made to do so. Similarly, failure at one sort of inquiry does not imply anything about one's failure or success at the other. As Weber noted "...just as the person who attempted to govern his mode of walking continuously by knowledge of anatomy would be in danger of stumbling so the professional "scientist" who attempted to determine the aims "and methods" of his own research extrinsically on the basis of methodological philosophy of science reflections would be in danger of falling into the same difficulties."<sup>36</sup>

"There is only one way of seeing one's own spectacles clearly and that is to take them off. It is impossible to focus both on them and through them" simultaneously.<sup>37</sup>

That the writings by behavioralists on the philosophy of political science hardly measure up to the best work they have done in behavioral political science does not allow us to judge either their particular research or philosophy of political science in general. Thus, while it may be agreed that Eulau's philosophy of political science is easily





defeated in argument, it must be remembered that he is not a philosopher of political science. There is no reason to suppose that what he takes as a satisfactory or defensible philosophy of political science would be what someone whose speciality was philosophy of political science would take to be a satisfactory philosophy of political science. It is conceivable that a philosophy of political science could be postulated that would secure the toleration of both Eulau and Wolin. As I have tried to show in Chapter Four above, a variety of theories always exist to explain a given phenomenon. Eulau's is not the only one. Neither is Wolin's. There are others, others which perhaps are more satisfying to a wider range of people and have more truth than any we have seen.

But, of course, no philosophy of political science can hide the character of contemporary science. Thus today, science cannot escape from those criticisms which attack its very constitution as undesirable, e.g. that it is non-transcendental in orientation, that it aims at justification, or that there is a narrow concentration of the time and energy of scientists in scientific communities. Science, however, is not to be judged by attacks that do not, in fact, understand its character. I suggest that science cannot be faulted for not being what it is not, and not doing what it cannot, or, by the same token, for being what it is. The responsibility for these matters lies in our use of science. We must be aware of its limitations. The characteristics of science take on especial meaning only insofar as they are interpreted or used, and such interpretations and uses are a product of particular men at particular times and not of science.

It seems, then, that scientists commonly will not know much about science but will be knowledgeable of scientific



1. The first step in the process of knowledge is the acquisition of information.

2. The second step is the organization of this information into a coherent system.

3. The third step is the application of this system to a specific problem.

4. The fourth step is the evaluation of the results of the application.

5. The fifth step is the refinement of the system based on the evaluation.

6. The sixth step is the repetition of the process for new information.

7. The seventh step is the integration of the new information into the existing system.

8. The eighth step is the application of the integrated system to a new problem.

9. The ninth step is the evaluation of the results of the application.

10. The tenth step is the refinement of the system based on the evaluation.

11. The eleventh step is the repetition of the process for new information.

12. The twelfth step is the integration of the new information into the existing system.

13. The thirteenth step is the application of the integrated system to a new problem.

14. The fourteenth step is the evaluation of the results of the application.

15. The fifteenth step is the refinement of the system based on the evaluation.

16. The sixteenth step is the repetition of the process for new information.

17. The seventeenth step is the integration of the new information into the existing system.

18. The eighteenth step is the application of the integrated system to a new problem.

19. The nineteenth step is the evaluation of the results of the application.

20. The twentieth step is the refinement of the system based on the evaluation.

21. The twenty-first step is the repetition of the process for new information.

22. The twenty-second step is the integration of the new information into the existing system.

23. The twenty-third step is the application of the integrated system to a new problem.

24. The twenty-fourth step is the evaluation of the results of the application.

25. The twenty-fifth step is the refinement of the system based on the evaluation.

26. The twenty-sixth step is the repetition of the process for new information.

27. The twenty-seventh step is the integration of the new information into the existing system.

28. The twenty-eighth step is the application of the integrated system to a new problem.

29. The twenty-ninth step is the evaluation of the results of the application.

30. The thirtieth step is the refinement of the system based on the evaluation.

work while philosophers of science commonly, will be knowledgeable of science but will not be knowledgeable of scientific knowledge. Hence, though philosophers of science are authorities on the nature of science, they are not authorities in how to do science. For it is at this point that Eulau is partially correct when he contends that "the study of politics" will not be made "'scientific'" by reflection on the nature of science or searches for the right theory or philosophy of science. It will become scientific only by the doing of science and this can be done only by scientists, not philosophers of science.<sup>38</sup> The possibility of something, in this case a science, developing can only be answered by the actual appearance of it. It is, in short, largely an empirical matter.<sup>39</sup>

Throughout the descriptions of the perspectives of the advocates and the critics on the notion of scientific method there has been constant reference to the idea that the activity of science may be distinguished from other activities by its having as its domain facts which are revealed by scientific method.

The term "science" is not applied to all forms of inquiry that may claim to be dealing in some way with facts nor can it be applied to all forms of inquiry that are in some widely acceptable sense accurate.<sup>40</sup> Facts are hard to escape. Failing to lie about them insofar as they are relevant to one's interest is not alone qualification for the appellation "science." Likewise failing to be careless and unsystematic is not alone such grounds.

"Science" does not equal a group of "facts" and a group of "facts" do not equal "science", though both advocates and critics often seem to think so.



Factual claims which are confirmed, or not disconfirmed, or have not as yet been disconfirmed are not necessarily an indication that they are a part of the activity of science. As we have seen in Chapter Four, the assertion of a conjecture about the world is not itself a sign of science, and furthermore even confirmation of a conjecture does not demonstrate that the process of the conjecture itself might profitably be thought of as scientific. Such conjectures may have just as easily have occurred in a dream. In the context of discovery, short of the context of justification, perhaps the only clue that the activity undertaken, or contemplated, might with some propriety be regarded as scientific is the intention of the individual involved to submit any conjecture he conceives to the rigors of the scientific context of justification.

Meehan's treatment of the factual aspects of science may be of particular interest, given that his work is of a philosophy of political science sort. As will be recalled, his view is that, though there are some epistemological reservations, essentially science is an enormous edifice consisting of logic and brute facts.<sup>41</sup>

In my judgment there are at least three problems in Meehan's position. In no particular order they are as follows: (1) "Epistmeology" is generally defined as "theory of knowledge." Science is both procedure and substance. One part of science as procedure is, as I have tried to show in Chapter Four, the context of justification. Science is concerned with a kind of knowledge, synthetic knowledge. The context of justification is concerned, in part, with how statements asserting synthetic propositions are justified. That is to say, by what criterion are they judged to be true and false. Insofar as this is the case, science itself implies, or carries, a partial notion of epistemology. To maintain that the development





of an epistemology clears the ground for science to observe "brute facts" may be therefore, problematic, for by begging off the question of epistemology the question of the nature of science is also begged to a certain extent. Science is more than "logical inference" and "brute facts". As we have seen, it involves basic conjectures as to the nature of sense phenomena, i.e. the domain of synthetic knowledge. Such conjectures or refutations may be tested and justified by logic and brute facts but they are not generated by them. (Indeed, logic and brute facts may, instead, be generated by them. This would make Meehan's position circular. The concept of science determines the concept of facts and logic. That facts and logic legitimated, if not created entirely, by the theory, i.e. conjectures, do not disconfirm the theory readily should not be surprising. If they did the theory would not have been accepted in the context of justification in the first place.) As a consequence no sharp distinction between epistemology and science may be advisable. But, even if I am mistaken, or if Meehan does not wish to say that scientists have an epistemology first and then do science based on it, two further problems may be raised. (2) Epistemologies do not achieve monopoly. It has often been shown that more than one scientific theory may be applied to any domain.<sup>42</sup> More than one epistemology, i.e. theory of knowledge, may be applied to any domain of knowledge, or knowledge generically. It is conceivable that "brute facts" may not turn out to be so brutish across a variety of epistemologies applied to a single domain. This might be done by different persons, or by the same person in different circumstances. (3) As we have seen in Hanson much of the perception of facts is conditioned by expectations.<sup>43</sup> Such expectations are not entirely comprised in an epistemology. Expectations for example arise from instructions, previous experience, training, or mistake. Standards of the constitution of factual knowledge may be shared by two persons and



yet they may not "see" the same "fact" when together presented with a sense phenomenon. By training, experience or instruction they may look for, and not surprisingly, therefore, see, different things -- different facts. Further as is easily shown, brute facts can be simply deceptive. Observation of facts without prior judgments is, as can be seen from Popper and Hanson, impossible.<sup>44</sup> For example, even Frohock's second point in his explication of scientific method admits this by stressing the importance of the application and reapplication of the same techniques by scientists.<sup>45</sup> The selection and application of such techniques in the first instance are judgements made prior to observation. These techniques effect both the structure and substance of observation. It is not surprising then that when the same techniques are utilized that the same facts are seen.

Very briefly I now want to review the three kinds of post-behavioral critiques in light of Chapter Four and the above comments in this chapter. I shall treat these critiques in the reverse order of my presentation of them.

The last post-behavioral critique, I maintained, describes science as having certain inherent characteristics which made it an inappropriate technique for the analysis of the domain of politics. Two things need be said about this position. First, if it implies that the study of the subject matter, politics, may be usefully comprised of more than sense phenomena, i.e. the subject matter suitable for science, then the point is well taken. If the study of politics needs to concern itself both with sense phenomena and more than sense phenomena then science and more than science should constitute the study of politics. But as we have seen, this is not Wolin's argument. He wishes to do away with scientific political science and substitute for it what he calls epic political theory so as to achieve, it seems the single





purpose of the study of politics -- scientific political knowledge.<sup>46</sup> Quite simply put, this position is illogical. Scientific knowledge, the product of science, cannot be derived from non-scientific inquiries (say for example, epic political theory if it were unscientific). This is true by definition. For to be labeled scientific knowledge an idea must withstand the context of justification, and as I have said, this is the one unmistakable sign of a science. Scientific knowledge must come from the doing of science. Insofar as epic political theory produces scientific knowledge it is a science, Wolin notwithstanding.

A second post-behavioral critique is that science has certain characteristics such that though it is useful for the study and understanding of sense phenomena of politics it stops with that alone making no reference to political actions. Political science is regarded therefore, as being quite limited, and perhaps even useless, in view of the political problems it is thought that we face. In this argument scientific knowledge is thought to be, in some sense, pure, i.e. removed from the contexts of action and value. It is hence, so the argument goes, not relevant to those spheres and, as those spheres are the only things of importance, political science and scientific political knowledge are not important, but largely useless or irrelevant.

Just as science has been criticized for not contributing to political action, so too it has been criticized for its abstractness, as we have seen in Cooper, Kress and Wolin.<sup>47</sup> The assumption seems to be that it is the purpose of science to reproduce reality. It is concluded that science is defective for it accomplishes no such thing due to its abstractness. The error is in the confusion of a description with what is described. "Albert Einstein once remarked, it is not the function of science 'to give the taste of soup'. To be a





description of the taste of soup is clearly not to be the taste of soup." "Surely," Rudner continues, there is "no reason whatever for anyone to suppose that a description of some characteristic of soup should, itself, taste like soup."<sup>48</sup>

The final post-behavioral critique is that which maintains that political science qua science, though possible and desirable, is at present misapplied. This critique takes exception to scientific political science in terms of its subject matter and not so much in terms of its methods. It is my view that in so arguing, Bay, for example, approaches Kuhn's image of the revolutionary in science, who, having a concern for the anomalies in the present scientific theories, seeks to direct science at a new range of questions formulated under a conjectured theory.<sup>49</sup> The revolutionary, in this case, Bay, as an example, hopes that the consideration of his proposed new questions will contribute to a greater understanding of the domain of his science.

The criticism that scientific political science is inappropriate may be, at best, a tonic to remind political scientists of the place of conjectures, perception and the implicit knowledge in science. The criticism that political science qua science is limited seems to be largely irrelevant for the political scientists except, as an even more rarely needed tonic, to stay the enthusiasm of scholars such as Eulau who wish, it seems, to scientize everything. The third critique is of much greater importance to active political scientists for it challenges the present conception of the domain of political science, presenting, in Kuhn's terms, a revolutionary potential. The issues this third critique raises are, rightly, much more likely to stimulate response from political scientists.<sup>50</sup>



In this chapter, I have shown that the perspectives on the notion of scientific method and science of the kinds discussed in Chapter Two and Three are, in light of Chapter Four, unsatisfactory.<sup>51</sup> This is the case because such perspectives either overlook entirely, or give only the scantiest attention to a part of science which I have called the context of discovery. This part of science is its creative imaginative side; it is most unmethodical.

I have concluded that the advocates of the idea of a political science have a problematic concept of science insofar as it is revealed by their notion of scientific method. Similarly, I have concluded that the opponents of the idea of a political science have a problematic concept of science.

More generally, it is my conclusion that the debate between the advocates of the idea of a political science and its critiques over the nature and use of science and scientific method is a pseudo-debate.<sup>52</sup> Political science qua science is not at stake, for neither group of contestants has an image of science that is compatible with those held by philosophers of science. The domain of study of philosophy of science is science, whereas that of the scientist is sense phenomena; hence philosophers of science may have a truer perspective on science than scientists themselves do. If neither of the groups of antagonists has a useful view of science then science can hardly be at stake in their contests. Therefore, regardless of which position wins the day it must be clear that the idea of a political science has not yet been properly introduced in the contest let alone judged by it. It should be noted that even those post-behavioralists who urge that scientific political science adopt a newly conceived subject matter share a view of science that allows them to speak of scientific method as rules of inquiry.





The danger to political science in this polemic lies in its misunderstanding of science, not in the possible triumph of one position or the other. The advocates of a political science qua science commit a two-step error in their effort to build a scientific political science. First, their perception of science is logically problematic at a number of points in view of the arguments of philosophers of science. Second, science cannot be achieved by emulating science, for clearly the natural sciences had no model to ape. If political science is to be like natural science the first thing political scientists must do is to stop looking over their shoulders at natural science.<sup>53</sup> In a way the error of the critics is two-step as well. First, their vision of the idea of a political science is limited to a distillation of what is currently practiced under that name. Second, insofar as they take this distillation to represent science, they share the advocates' mistaken view of the character of science.

To the extent that the views of the advocates of a political science considered are useful they refer only to science in the context of justification, which embraces only part and not all of science. To the extent that the charges of the critics considered are useful they are more nearly a function of individual political scientists than science. "Clearly, much of the new political science is pervaded by a maddening rush to produce nonsense. But, then again, much of the old political science was nonsense, too, even if invoked at a more leisurely pace." In this case one should be careful of throwing out the baby of science with the bath water of its imperfections.<sup>54</sup> I believe that the criticism levelled by the post-behavioralists at the idea of a political science should instead be re-directed at those political scientists, of whatever persuasion, who have slipped into certain facile assumptions about the notion of scientific method and the idea of a political science.



FOOTNOTES, CHAPTER FIVE

<sup>1</sup>M. Sibley, "Behavioralism," pp. 56-58; S. Wolin, "Political Theory," pp. 1070-1071; M. Polanyi, Man, pp. 12, and passim; H. Eulau, Behavioral Persuasion, p. 9; and H. McClosky, Inquiry, p. 9.

<sup>2</sup>A. Brecht, "Theory," p. 308.

<sup>3</sup>See, e.g., S. Levinson, "Notes on the Notion of 'Objectivity' in the Teaching of Political Science," paper read at a Seminar on the Caucus for a New Political Science at the Center for the Study of Democratic Institutions, 1969, p. 6.

<sup>4</sup>Polanyi, Man, p. 26.

<sup>5</sup>Brecht, "Theory," p. 308.

<sup>6</sup>Wolin, "Political Theory," pp. 1071 and 1077.

<sup>7</sup>H. Reichenbach, Experience, p. 7.

<sup>8</sup>Ibid. and Polanyi, Man, p. 21 - 22.

<sup>9</sup>Wolin, "Political Theory," pp. 1063-1064 and 1071.

<sup>10</sup>B. Rockman, "A 'Behavioral' Evaluation of the Critique of Behaviorism", Paper read at the American Political Science Association Annual Meeting (1969), p. 8.

<sup>11</sup>Wolin, "Paradigms," p. 132.

<sup>12</sup>T. Kuhn, Scientific Revolutions, p. 5.

<sup>13</sup>N. Hanson, Patterns, pp. 5 and 30.

<sup>14</sup>Wolin, "Political Theory," p. 1081.

<sup>15</sup>McClosky, Inquiry, p. 9.

<sup>16</sup>N. Jacobson, "Values and Science in Political Theory," Paper read before the Conference on Reason and Value of the Pacific Coast Committee for the Humanities, American Council of Learned Societies, Mills College, 1952; cited in D. Waldo, "'Values' in Political Science," R. Young, ed., Approaches, p. 110, F. 12.

<sup>17</sup>F. Frohock, Political Inquiry, p. 110.





<sup>18</sup>H. Eulau, Behavioral Persuasion, pp. 9 and 114.  
Cf. Eulau, "Tradition", p. 15.

<sup>19</sup>Wolin, "Political Theory", pp. 1064, 1072, 1082  
and passim.

<sup>20</sup>Ibid., p. 1082.

<sup>21</sup>Hanson, Patterns, p. 36.

<sup>22</sup>Wolin, "Political Theory", pp. 1070, 1072, 1074  
and passim. Cf. "Paradigms", passim.

<sup>23</sup>For some remarks by Wolin on his interest in science  
see J. Walsh, "Behavioral Sciences", Science, CLXIX (1970),  
p. 657. Wolin may also be using these ideas to think about  
politics as well, see, e.g., "Paradigms and Political Theories",  
p. 149. Cf. T. Thorson, Review of Politics and Experience  
edited by King and Parekh, American Political Science Review,  
LXIII (1969), p. 935. This notion has been attempted previous-  
ly. See R. Pranger, The Eclipse of Citizenship (New York:  
Holt, 1968), pp. 91 - 92.

<sup>24</sup>Wolin is aware of a view of science which emphasizes  
its creative aspect:

'Science' -- to use Hobbes's comprehensive term -- had  
progressed as rapidly because scientists had been bold  
enough to break with traditional modes of thought and  
inquiry. They had refused to follow the path of building  
slowly on past achievements, zealously preserving the  
main corpus and modifying only where necessary. The un-  
precedented development of 'science' was pictured by  
Hobbes as an intellectual drama of creative destruction...  
By intellect alone, without appeal to superhuman authority  
and without relying upon non-rational and non-sensory  
faculties, man had created a rationally intelligible  
cosmos without mystery and occult qualities.

Politics, p. 245. The reference to Hobbes could be to  
Leviathan, edited with an introduction by M. Oakeshott (Oxford:  
Blackwell, 1961), pp. 29 - 30.

<sup>25</sup>C. Gillispie, "The Nature of Science", Science,  
CXXXVIII (1962) p. 1253.

<sup>26</sup>Wolin and J. Schaar, Review Essay of Essays in the  
Scientific Study of Politics edited by J. Storing, American  
Political Science Review, LVII (1963), p. 125.

<sup>27</sup>Wolin, "Political Theory", p. 1064.





- <sup>28</sup>S. Toulmin, Foresight, p. 57.
- <sup>29</sup>Wolin, "Political Theory", p. 1077.
- <sup>30</sup>Ibid., p. 1070.
- <sup>31</sup>Ibid., p. 1071.
- <sup>32</sup>Ibid., p. 1077.
- <sup>33</sup>Toulmin, Foresight, p. 13. Cf. N. Jacobson, "Unity", p. 115.
- <sup>34</sup>See T. Hobbes, Leviathan, pp. 57 and 115.
- <sup>35</sup>F. Oppenheim, Moral Principles and Political Philosophy (New York: Random House, 1968), p. 15.
- <sup>36</sup>M. Weber, Methodology, p. 115.
- <sup>37</sup>Toulmin, Foresight, p. 57.
- <sup>38</sup>Eulau, Behavioral Persuasion, p. 9.
- <sup>39</sup>See e.g., C. Hyneman, The Study of Politics (Urbana, Illinois: University of Illinois Press, 1959), p. 157; Cf. K. Popper, Discovery, p. 205-206.
- <sup>40</sup>See e.g., G. Catlin, The Science and Method of Politics (New York: Knopf, 1927), pp. 91 and 94 and Systematic Politics (Toronto: University of Toronto Press, 1962), p. 5.
- <sup>41</sup>E. Meehan, Theory and Method, p. 39.
- <sup>42</sup>Kuhn, Scientific Revolutions, p. 76 and 93.
- <sup>43</sup>Hanson, Patterns, p. 15.
- <sup>44</sup>Ibid., Popper, Historicism, p. 134-135, and Toulmin, Philosophy, pp. 46 and 66.
- <sup>45</sup>Frohock, Political Inquiry, p. 110.
- <sup>46</sup>Wolin, "Political Theory", p. 1082.
- <sup>47</sup>Ibid., B. Cooper, "Behavioralism", p. 27 and P. Kress, "Politics", p. 10.
- <sup>48</sup>R. Rudner, Philosophy, p. 69.
- <sup>49</sup>Kuhn, Scientific Revolutions, pp. 84, 92 and passim.



<sup>50</sup>See, e.g., the papers in Eulau, ed., Tradition.

<sup>51</sup>Cf. Ellen and Neal Wood, "Canada and the American Science of Politics," I. Limseden, ed., Close the 49th Parallel Etc. (Toronto: University of Toronto Press, 1970), p. 182. Present conceptions of science and scientific method held in the new political science are "simplistic, restricted and shallow". "These conceptions resemble notions abandoned by the natural sciences in the last century....it is often not because of its much vaunted preoccupation with facts, but precisely in order to rescue facts that many critics attack the narrow superficiality of much of contemporary political science."

<sup>52</sup>For an example of such a pseudo-debate see M. Haas and T. Becker, "A Multimethodological Plea", Polity, II (1970), pp. 267-294; G. Schubert, "The Third Cla't Theme", Political Science, II (1969), pp. 591-597; M. Haas, "Three Types of Science", Ibid., pp. 598-599; and F. Dallymer, "Empirical Political Theory and the Image of Man", Polity, II (1970), pp. 443-478.

<sup>53</sup>See e.g., F. A. Hayek, The Counter Revolution of Science (New York: The Free Press, 1955), p. 14 and J. Brazeau, "A Social Scientist's View", Science Forum, II (1969), p. 13. It has recently been proposed that political science model itself after the applied natural sciences, such as for instance medicine, rather than the pure natural sciences, D. MacRae, Jr., "Social Science and the Source of Policy", Political Science, III (1970), p. 308. Aside from my own reservations about the epistemological significance of the distinction between pure and applied natural sciences, the argument, though interesting, still entails emulation offering only a new idol.

<sup>54</sup>Rockman, "Evaluation of the Critique", p. 25. Cf. J. T. Bookman, "The Disjunction of Political Science and Political Philosophy", American Journal of Economics and Sociology, XXIX (1970), pp. 17-24. Surkin attempts to counter Rockman's position. See M. Surkin, "Sense and Nonsense", p. 574.





## BIBLIOGRAPHY

### Books -

- Almond, Gabriel and Verba, Sidney. The Civic Culture. Princeton: Princeton University Press, 1963.
- Ashford, T. The Physical Sciences. 2nd ed. Revised. New York: Holt, 1967.
- Bay, C. The Structure of Freedom. New York: Athean, 1965 (1957).
- Berger, Peter and Luckman, Thomas. The Social Construction of Reality. Garden City, N.Y.: Doubleday, 1966.
- Brecht, Arnold. Political Theory: The Foundations of Twentieth-Century Political Thought. Princeton: Princeton University Press, 1959.
- Bridgeman, Perry W. The Logic of Modern Physics. New York: Macmillan, 1928.
- Campbell, N. What Is Science. London: Meuthen, 1921.
- Carnap, R. Philosophical Foundations of Physics. Edited by M. Gardner. New York: Basic Books, 1966.
- Carroll, J., editor Language, Thought and Reality. New York: Wiley, 1956.
- Catlin, George. The Science and Method of Politics. New York: Knopf, 1927.
- Catlin, George. Systematic Politics. Toronto: University of Toronto Press, 1962.
- Churchman, C. Predictions and Optimal Decisions. Engelwood Cliffs, New Jersey: Prentice-Hall, 1961.
- Conant, J. Modern Science and Modern Man. New York: Columbia University Press, 1952.
- Coombs, C. A Theory of Data. New York: Wiley, 1964.
- Crick, Bernard. The American Science of Politics. London: Routledge & Kegan Paul, 1959.
- DeGrazia, Alfred. Political Behavior. Second edition. New York: The Free Press, 1962.



- DeGrazia, Alfred. Political Organization. Second edition. New York: The Free Press, 1962.
- Dewey, John. Logic: The Theory of Inquiry. New York: Holt, 1938.
- Easton, D. A Framework for Political Analysis. Engelwood, Cliffs, New Jersey: Prentice Hall, 1965.
- Easton, D. The Political System. New York: Knopf, 1953.
- Easton, D. Systems Analysis of Political Life. New York: Wiley, 1965.
- Eulau, H. The Behavioral Persuasion in Politics. New York: Random House, 1963.
- Frohock, Fred. The Nature of Political Inquiry. Homewood, Illinois: Dorsey Press, 1967.
- Golembiewsk, Robert, Welsh, William and Crotty William. A Methodological Primer for Political Scientists. Chicago: Rand McNally, 1969.
- Grunbaum, A. Philosophical Problems of Space and Time. New York: Knopf, 1963.
- Hagstorm, W. The Scientific Community. New York: Basic Books, 1965.
- Hanson, Norwood R. Patterns of Discovery. Cambridge: Cambridge University Press, 1965.
- Hampden-Turner, Charles. Radical Man. Cambridge, Massachusetts: Schenkman, 1970.
- Hayek, Fredrick. The Counter Revolution in Science. New York: The Free Press, 1955.
- Hempel, Carl. Philosophy of Natural Science. Engelwood Cliffs, New Jersey: Prentice Hall, 1966.
- Hobbes, Thomas. Leviathan. ed. with an introduction by Michael Oakshott. Oxford: Blackwell, 1651.
- Hume, David. Treatise on Human Nature. Two volumes. London: Dent, 1817.
- Hyneman, Charles. The Study of Politics. Urbana, Illinois: University of Illinois Press, 1959.





- Isaak, Alan. The Scope and Method of Political Science. Homewood, Illinois: Dorsey Press, 1969.
- Kaplan, Abraham. The Conduct of Inquiry. San Francisco: Chandler, 1964.
- Kaufman, Arnold. Methodology of the Social Sciences. New York: Humanities Press, 1958.
- Kuhn, Thomas S. The Structure of Scientific Revolutions. Second edition. Chicago: University of Chicago Press, 1969.
- Lasswell, Harold. Power and Personality. New York: Viking Books, 1948.
- Mannheim, Karl. Ideology and Utopia. Translated by L. Wirth and E. Shils. New York: Harcourt, 1936 (1929).
- Marcuse, Herbert. One-Dimensional Man. Boston: Beacon Press, 1965.
- McClosky, Herbert. Political Inquiry. New York: Macmillan, 1969.
- McRae, J.S., Duncan. "Social Science and the Sources of Policy." Political Science. III (1970), pp. 294-311.
- Meehan, Eugene. Explanation in Social Science. Homewood, Illinois: Dorsey Press, 1968.
- Meehan, Eugene. The Theory and Method of Political Analysis. Homewood, Illinois: Dorsey Press, 1965.
- Merriam, Charles. New Aspects of Politics. Second edition. Chicago: University of Chicago Press, 1931.
- Murphy, Joseph. Political Theory: A Conceptual Analysis. Homewood, Illinois: Dorsey Press, 1968.
- Oppenheim, Felix. Moral Principles and Political Philosophy. New York: Random House, 1968.
- Pennock, J. Roland and Smith, David. Political Science: An Introduction. New York: Macmillan, 1964.
- Popper, Karl. Conjectures and Refutations. London: Routledge and Kegan Paul, 1963.
- Popper, Karl. Logic of Scientific Discovery. London: Hutchinson, 1959.





- Popper, Karl. Poverty of Historicism. Second edition. London: Routledge and Kegan Paul, 1961.
- Polanyi, Michael. Personal Knowledge. Chicago: University of Chicago Press, 1958.
- Polanyi, Michael. The Study of Man. London: Routledge and Kegan Paul, 1958.
- Pranger, Robert. The Eclipse of Citizenship. New York: Holt, 1968.
- Reichenbach, Hans. Experience and Prediction. Chicago: University of Chicago Press, 1938.
- Reichenbach, Hans. The Rise of Scientific Philosophy. Los Angeles and Berkeley: University of California Press, 1966.
- Runciman, W.G. Political Theory and Social Science. Cambridge: Cambridge University Press, 1963.
- Rudner, Richard. Philosophy of Social Science. Engelwood Cliffs, New Jersey: Prentice Hall, 1966.
- Scheffler, Israel. Science and Subjectivity. Indianapolis: The Bobbs-Merrill Press, 1967.
- Storer, N. The Social System of Science. New York: Holt, 1966.
- Thorson, Thomas L. Biopolitics. New York: Holt, 1970.
- Toulmin, S. Philosophy of Science. Cambridge: Cambridge University Press, 1961.
- Toulmin, S. Foresight and Understanding. London: Hutchinson, 1961.
- Van Dyke, Vernon. Political Science: A Philosophical Analysis. Stanford: Stanford University Press, 1960.
- Watson, James. The Double Helix. New York: Atherton, 1968.
- Weber, M. The Methodology of the Social Sciences. Translated and edited by E. Shils and H. Finch with a forward by Shils. Glencoe, Ill.: The Free Press, 1949.
- Weise, P. The Science of Biology. New York: McGraw-Hill, 1959.



Wolin, Sheldon. Politics and Vision. Boston: Little and Brown, 1960.

Ziman, John. Public Knowledge: An Essay Concerning the Social Dimension of Science. Cambridge: Cambridge University Press, 1968.

#### Articles and Periodicals -

Barber, Bernard. "Resistance by Scientists to Scientific Discovery," Science, CXXXIV (1961), pp. 596-602.

Bay, C. "Liberalism," Centennial Review of Arts and Sciences, IV (1960), pp. 331-353.

Bay, C. "Politics and Pseudopolitics," American Political Science Review, LIX (1965), pp. 39-57; reprinted in Apolitical Politics, edited by Charles McCoy and John Playford, pp. 12-37. New York: Crowell, 1967.

Bay, C. "The Cheerful Study of Dismal Politics," The Dissenting Academy, edited by Theodore Roszak, pp. 208-230. New York: Pantheon, 1968.

Bay, C. "Needs, Wants, Desires and Political Legitimacy," Canadian Journal of Political Science, I (1968), pp. 241-260.

Bookman, John T. "The Disjunction of Political Science and Political Philosophy," American Journal of Economics and Sociology, XXIX (1970), pp. 17-24.

Boyer, C. "Commentary on the Paper of T. S. Keehn," Critical Problems in the History of Science, edited by M. Clagett, pp. 384-390. Madison: University of Wisconsin Press, 1958.

Brazeau, J. "A Social Scientists View," Science Forum, II (1969), pp. 12-13.

Brecht, A. "Political Theory: Approaches," International Encyclopedia of the Social Sciences, XII, edited by David Sills, pp. 307-318. New York: Macmillan and the Free Press, 1968.

Brown, R. and Lenneberg, E. "A Study in Language and Cognitions," Journal of Abnormal and Social Psychology, IL (1954), pp. 454-462.





- Dahl, Robert. "The Behavioral Approach in Political Science," American Political Science Review, LV (1961), pp. 763-772; reprinted in Politics and Social Life, edited by N. Polsby, R. Dentler and P. Smith, pp. 15-25. Boston: Houghton Mifflin, 1963.
- Dallymer, Fred. "Empirical Political Theory and the Image of Man," Polity, II (1970), pp. 443-478.
- DeGrazia, A. "The Politics of Science and Dr. Verlitosky," American Behavioral Scientist, VII, 1963, pp. 45-50 and 53-68.
- Deutsch, Karl. "Recent Trends in Research Methods in Political Science," A Design for Political Science, edited by J. Charlesworth, pp. 149-178. Philadelphia: Association of the Political and Social Sciences, 1966.
- Deustch, Karl, and Rieslebach, Leroy. "Recent Trends in Political Theory and Political Philosophy," Annals of the American Academy of the Political and Social Sciences, CCCLX (1965), pp. 139-162.
- Dreyer, F. and Rosenbaum, W. "The Study of Public Opinion and Electoral Behavior," Public Opinion and Electoral Behavior, edited by Dreyer and Rosenbaum. Belmont, Calif.: Wadsworth, 1966, pp. 1-11.
- Easton, D. "The Current Meaning of Behavioralism," Contemporary Political Analysis, edited by J. Charlesworth, pp. 11-31. New York: The Free Press, 1967.
- Easton, D. "The New Revolution in Political Science," American Political Science Review, LXIII (1969), pp. 1051-1061.
- Easton, D. "Political Science," International Encyclopedia of the Social Sciences, V. XII, edited by D. Sills, pp. 282-298. New York: Macmillan and the Free Press, 1968.
- Ebenstein, W. Book review of Political Theory, by A. Brecht, Annals of the American Acedemy of the Political and Social Sciences, CCCLVI (1959), p. 171.
- Eulau, H. "The Behavioral Movement in Political Science," Social Research, XXXV (1968), pp. 1-29.
- Eulau, H. "Comment on Deutsch," A Design for Political Science, edited by J. Charlesworth, pp. 179-184. Philadelphia: American Academy of Political and Social Science, 1966.



- Eulau, H. "Political Behavior," International Encyclopedia of the Social Sciences, V. XII, edited by David Sills, pp. 203-214. New York: Macmillan and the Free Press, 1968.
- Eulau, H. "Segments of Political Science Most Susceptible to Behavioristic Treatment," Contemporary Political Analysis, edited by J. Charlesworth, pp. 32-50. New York: The Free Press, 1967.
- Eulau, H. "Tradition and Innovation," Behavioralism in Political Science, edited by Eulau, pp. 1-21. New York: Atherton, 1969.
- Gillispie, C. "The Nature of Science," Science, CXXXVII (1962), pp. 1251-1253.
- Gumperez, J.J. "Language and Communication," Annals of the American Academy of the Political and Social Sciences, CCCLXXIII (1967), pp. 219-231.
- Gunnell, John. "Deduction, Explanation and Social Scientific Inquiry," American Political Science Review, LXIII (1969), pp. 1233-1246.
- Gunnell, John. "Social Science and Political Reality," Social Research, XXXV (1968), pp. 151-202.
- Haas, Michael and Becker, Theodore. "A Multimethodological Plea," Polity, II (1970), pp. 267-294.
- Handy, R. and Kurtz, P. "A Current Appraisal of the Behavioral Sciences," American Behavioral Scientists, VII (1963-64), supplement.
- Jacobson, Norm. "The Unity of Political Theory," Approaches to the Study of Politics, edited by R. Young, pp. 155-124. Evanston, Illinois: Northwestern University Press, 1958.
- Kariel, Henry. "Expanding the Political Present," American Political Science Review, LXIII (1969), pp. 768-776.
- Key, V.O. "The Politically Relevant in Surveys," Public Opinion Quarterly, XXIV (1960), pp. 54-61.
- Kress, Paul. "Politics and Society," Polity, II (1969) pp. 1-13.
- Kress, Paul. "Self, Society and Significance," Ethics, LXXVII (1966), pp. 1-13.





- Kuhn, Thomas S. "The Essential Tension: Tradition and Innovation in Scientific Research," Scientific Creativity, edited by C. W. Taylor and F. Barron, pp. 341-354.
- Kuhn, Thomas S. "The Function of Measurement in Modern Physical Science," Isis, LII (1961), pp. 161-193.
- Kuhn, Thomas S. "Historical Structure of Scientific Discovery," Science, CXXXVI (1962), pp. 760-769.
- Lijphart, Arend. "Political Science vs. Political Advocacy," Acta Politica, V (1970), pp. 165-171.
- McCoy, Charles and Playford, John. "Introduction," Apolitical Politics, edited by McCoy and Playford, pp. 1-10.
- Mehlberg, H. "The Range and Limits of the Scientific Method," Journal of Philosophy, LI (1954), pp. 285-294.
- Merkel, Peter. "'Behavioristic' Tendencies in American Political Science," Behavioralism in Political Science, edited by H. Eulau, pp. 141-153. New York: Atherton, 1969.
- Merton, Robert. "Behavior Patterns of Scientists," American Scholar, (1969), pp. 197-225.
- Pinner, Frank. "Notes on Method in Social and Political Research," Politics and Social Life, edited by N. Polsby, R. Dentler and P. Smith, pp. 145-163. Boston: Houghton Mifflin Co., 1963.
- Polanyi, Michael. "The Growth of Science in Society," Minerva, V, (1967), pp. 533-545.
- Polanyi, Michael. "The Potential Theory of Absorption: Authority in Science Has Its Uses and Its Dangers," Science, CXXXVI (1962), pp. 760-769.
- Polanyi, Michael. "The Republic of Science," Minerva, I (1960), pp. 54-73.
- Popper, Roger. "'The Time Has Come' (The Walrus Said)," Western Canadian Journal of Anthropology, I (1970) pp. 12-34.
- Schubert, Glendon. "The third Clait Theme: Wild in the Corridors," Political Science, II (1969), pp. 591-597.





- Sherif, Muzfar. "Group Influence Upon the Formation of Norms and Attitudes," Readings in Social Psychology, edited by T. W. Newcomb and E. Harley, pp. 77-89. New York: Holt, 1947.
- Sibley, Mulford Q. "The Limitations of Behavioralism," Contemporary Political Analysis, edited by J. Charlesworth, pp. 51-76. New York: The Free Press, 1967.
- Schutz, C.E. "Significance and Action in Social Science," Ethics, LXXII (1963), pp. 233-246.
- Spitz, David. "Politics and the Critical Imagination," Review of Politics, XXXII (1970), pp. 419-435.
- Storer, N. The Social System of Science, New York: Holt, 1966.
- Surkin, Marvin. "Sense and Nonsense in Politics," Political Science, II (1969), pp. 573-581.
- Taylor, Charles. "Neutrality in Political Science," Philosophy, Politics and Society, Third Series, edited by P. Laslett and W.G. Runciman, pp. 25-57. Oxford University Press, 1965.
- Thorson, T.L. Review of Politics and Experience, edited by P. King and C. Parekh, American Political Science Review, LXIII (1969), p. 935.
- Ulmer, Sidney. "Scientific Method and the Judicial Process," American Behavioral Scientists, VII (1963), pp. 21-28.
- Waldo, Dwight. "'Values' in the Political Science Curriculum," Approaches to the Study of Politics, edited by R. Young, pp. 96-111. Evanston, Illinois: Northwestern University Press, 1958.
- Walsh, J. "The Behavioral Sciences," Science, CLXIX (1970), pp. 654-658.
- Watson, J. and Crick, F. "Molecular Structure of Nucleic Acids," Nature, CLXXI (1953), pp. 737-738.
- Weber, Maxim. "Science as a Vocation," From Max Weber, translated, introduced and edited by C.W. Mills and H. Gerth, pp. 129-156. New York: Oxford University Press, 1946.
- Wolfe, Alan and Surkin, M. "The Political Dimension of American Political Science," Acta Politica, V (1970), pp. 43-61.
- Wolin, S. Letter to the Editor, American Political Science Review, LXIV (1970), p. 592.



Wolin, S. "Paradigms and Political Theories," Politics and Experience, edited by P. King and C. Parekh, pp. 125-152. Cambridge: Cambridge University Press, 1968.

Wolin, S. "Political Theory as a Vocation," American Political Science Review, LXIII (1969), pp. 1062-1082.

Wolin, S. and Schaar, John. A Review Essay of Essays in the Scientific Study of Politics, edited by H.J. Storing, American Political Science Review, LVII (1963), pp. 125-150.

Wood, Ellen and Neal. "Canada and the American Science of Politics," Close the 49th Parallel, edited by I Limseden, pp. 179-196. Toronto: University of Toronto Press, 1970.

#### Unpublished Material -

Cooper, Barry. "Behavioralism, Pluralism and Criticism," Paper read at Canadian Political Science Association Meetings, 1970.

Goodman, Paul. "Mass Education in Science," John Adams Lecture at U.C.L.A., 19 April 1966.

Jackson, M. "Social Research and Social Policy," unpublished paper, Alberta Human Resources Research Council, mimeograph, Edmonton, 1969.

Kalleberg, Arthur. "An Analysis of the Nature and Validity of the Idea of a Science of Politics in Recent Political Theory," unpublished PhD dissertation, Dept. of Political Science, University of Minnesota, 1960.

Levinson, Sanford. "Notes on the Notion of 'Objectivity' in the Teaching of Political Science," paper read at a seminar on the Caucas for a New Political Science at the Center For the Study of Democratic Institutions.

Mark, Max. "Empiricism as the Methodology of Conservatism," paper read at the American Political Science Association meetings, 1968.

Rockman, Bert. "A 'Behavioral' Evaluation of the Critique of Behavioralism," paper read at the American Political Science Association meetings, 1969.







**B29974**